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# The Canadian Medical Association Journal

*With which is incorporated the MONTREAL MEDICAL  
JOURNAL and the MARITIME MEDICAL NEWS*



EDITED BY  
ANDREW MACPHAIL  
MONTREAL

OLD SERIES VOL. XL

NEW SERIES VOL. I

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# The Canadian Medical Association Journal

ANDREW MAOPHAIL, Editor

216 Peel St., Montreal

Yearly in Advance \$5.00

Single Copies, 50c.

Remittances, advertisements or business communications are to be addressed to THE CANADIAN MEDICAL JOURNAL, 63 Bay Street, Toronto; all others to the Managing Editor, 216 Peel Street, Montreal. A limited number of reprints of articles will be furnished to authors at cost price, if a request to that effect accompany the manuscript.

VOL. I.

TORONTO, JANUARY, 1911

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Andrew Macphail, 216 Peel St., Montreal.

# The Canadian Medical Association Journal

VOL. I.

JANUARY, 1911

No. 1

## THE ANCIENT FOUNDATIONS OF HEREDITY

BY A. B. MACALLUM, PH.D., SC.D., F.R.S.

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IN selecting as the subject of my address the ancient foundations of heredity I have been influenced by a desire to use the opportunity to help in clarifying ideas about a matter which is not too clearly understood.

About six months ago a paper was published by Professor Karl Pearson and Miss Elderton upon the effect of the alcohol habit in heredity; and it was pointed out that the statistics available showed that alcohol used in excess by one generation does not necessarily result in deterioration of the next or a more remote generation. This conclusion was much criticised, and it was held that the evidence on which it was based was too slender, or of too narrow a scope, to support such a startling view. With this criticism it is possible to agree; but not a few of the critics assumed that heredity is, at foundation, a function which can be affected by adventitious influences and that, therefore, immediately acquired vicious habits in the matter of food and drink indulged in by the people of one generation seriously affect the physique and psychological character of their offspring, immediate or remote. According to this view such agents as alcohol and tobacco, when used to excess, directly influence heredity and result in physical and moral degeneration.

Now, there is no doubt that degeneracy is to a certain degree associated with alcoholism. It is also a fact that the nations of civilization, without an intention to that end, are making a gigantic experiment in regard to the effect of alcohol on the race, the result of which will not be determined in this or in many generations from now. There is,

Opening address of the Medical Faculty of the University of Liverpool, October 4th, 1910.

further, no reason to question the opinion that degeneration, formerly subject to the law of the survival of the fittest, is now on the increase in the denser centres of population, and it is disquieting to learn that the one-quarter of this nation which produces one-half of the children contains the vast majority of the degenerate class

It is, however, well to ascertain the true bearings of all these facts on each other before we associate any, or all, of them in the relation of cause and effect; for it is only through a right appreciation of the forces involved in bringing about the disquieting results that we can succeed in discovering an effective remedy.

Now, it is not at all likely that the biologist and the physiologist will confuse the situation. They, from their studies of animal and vegetable forms, recognize that heredity is a force which is affected by external, physical or chemical conditions, only after these have acted on the organisms for a long period of time, and that to the steady, stable character of this force is due the more or less pronounced fixity of type in species, genera, and races in the animal and vegetable kingdoms. There is a school, it is true, which maintains, as its cardinal doctrine, that heredity is not necessarily a stable and certain force, and that through this instability new species may suddenly arise, and facts are adduced which apparently support this opinion. But this school numbers only a few disciples, and the influence on biological thought which it exerts is correspondingly limited. The doctrine of heredity as an invariable force dominates in biological philosophy.

This doctrine is not an ancient one. Forty years ago it was practically unknown. Darwin in his "Theory of Pangenesis," advanced in 1873, appears to have accepted the view that external conditions directly affect the germ plasm of a species. It was only in the early "eighties" that Weismann disposed of this view, and established the immutability of the germinal material, except in a manner that is in no way due to the action of external forces. Although his doctrine has been keenly criticised, and although he has, as a result of the demonstration of weak points in it, modified somewhat its rigidity, it is accepted by the great majority of biologists.

This doctrine is not yet thirty years old, and in consequence it is not surprising that it has not yet made its way into the general thought of to-day. It is, indeed, not adequately grasped by the world of science itself, for one finds here and there evidence of a tacit recognition of the view that the germ plasm can be affected in a single generation by external conditions. An example of this is found in a recent contribution of Oscar Hertwig in which he appears to accept the unscientifically determined view of specialists in neurological medicine, that chronic

alcoholism in an individual does alter the heredity-bearing properties of his germ plasm.

It is of the greatest importance that right views should prevail amongst non-scientific thinkers and critics regarding the principles of heredity, for it is only in this way that they may be fully prepared to face the problems which profoundly affect racial welfare. There can be no remedy, except an empirical one, against disease, unless we know the causation of disease; and this knowledge can only be obtained after the principles which determine, or control, the normal condition have been ascertained. Racial degeneration is a disease, and its control, or cure, therefore, demands, primarily, a thorough understanding of the forces that contribute to, or are responsible for, the normal conditions. Those, therefore, who are students of social and economic conditions and of the effects of these in promoting racial degeneration must, first of all, thoroughly comprehend the stability of the force of heredity, if the results of their efforts are to be of service in checking one of the greatest dangers to civilization.

In consequence of these considerations, I have thought it advisable to draw attention to the question on this occasion and to put in a more or less succinct way the salient aspects of the great law of heredity. To do this without going over some familiar ground is of course not possible; but it seemed to me that the question might have a new interest if it were treated from an unusual point of view. That fresh point of view may be found in the study of how heredity arose and how it has been maintained throughout all the history of life on this earth. This comprehends all that is involved in the ancient foundations of heredity, and it is this which shall be my theme during the remainder of this address.

The duration of life has been variously estimated from twenty million years up to a thousand million years. The tendency, as our knowledge widens, has been to postulate a longer and longer period, and to-day those who have given attention to the subject hesitate rather at the acceptance of a short period than at the assumption of a long one. This would make one hundred millions of years a conservative estimate. If we accept this as safe, we may still regard it as a sweep of time of enormous length in which many things could have happened to influence in a remarkable degree the course of the history of living material. What living organisms are to-day is the result of all the forces which acted for a century of millions of years at least.

In this enormously long period changes were brought about in the structure and character of the cell, which constituted valuable adaptations to its environment and developed the function of heredity.

What these were are of moment, for they throw a clarifying light on the forces that have made for heredity. Those changes are, however, only to be comprehended through a careful study of all the types of animal and vegetable cells. This includes not only the structure, but also the chemistry and physics of the cell, and it is, therefore, necessary, before developing this theme, to review a few of the leading facts bearing on the structure of the cell as it is to-day.

The cell, the smallest unit of life, is formed of what is known as protoplasm. This consists of different kinds of proteins, and these are the most complicated compounds known. Protoplasm in its typical state is capable of growth, repair, and all the other activities and manifestations characteristic of living matter. This and its peculiar chemical and physical constitution, which it is endowed with the power to control, make it a unique phenomenon in the world of matter. Indeed, its very life is dependent on its power to maintain a large measure of independence of the world without itself.

Assisting in the maintenance of this individuality, and enclosing the whole mass, there is at least one membrane which differs in character in different cells. In the vegetable cell there may be two such membranes, one external, usually very thick and consisting of cellulose or allied carbohydrate material, the other very delicate, almost imperceptible and immediately limiting the protoplasm from which it differs in composition slightly but differs in properties in a very marked degree. This is the only membrane in animal cells as a rule, and in both animal and vegetable cells it is endowed with special properties. Through one of these the membrane is impermeable, to a certain degree, to salts or inorganic elements present in the surrounding fluid; because of this the cell is in great measure protected against its environment. It is also because of this impermeability that the cell retains in its interior not only its own proteins but also its own dissolved and digested food stuffs. The membrane, owing to this property of impermeability, has played an enormous part in the life of cellular organisms in the struggle for existence.

Situated more or less centrally in the protoplasm which constitutes the bulk of the cell, there is what is called a nucleus. This is a more or less spherical body which is enclosed by a membrane of its own, whose properties I shall discuss later. The material included in the nucleus is chiefly formed of a substance known to the biologist and cytologist as *chromatin*, to the chemist as *nucleo-protein*. The name *chromatin* was given to this substance because of the property it possesses of absorbing the staining matters or dyes used in the preparation of cellular material for microscopic examination. A *nucleo-protein*, on



the other hand, is formed of a protein and a remarkable combination of phosphoric acid united with bodies, known as purins and pyrimidins, through sugar molecules acting as connecting links.

As the bio-chemist prepares this nucleo-protein it is comparatively simple, but we must not conclude that it represents wholly the chromatin of the histologist. In the method of preparation of nucleo-protein the chromatin must undergo very considerable changes, and the nucleo-protein thus obtained can only be held to represent in a general way the original material, and no more, in fact, than the skeleton of the vertebrate, freed from all the soft attached parts, represents the living animal. The chromatin of the cytologist must, accordingly, be regarded as an exceedingly complicated compound, perhaps much more complicated than any other compound found in the living cell, and to this must be due the extraordinary rôle that chromatin plays in the life of the cell.

The evidence which recent studies on the cell have furnished has on the whole indicated that this chromatin is not formed in the nucleus but in the cytoplasm, that is, in the protoplasm outside of the nucleus. The latter is, therefore, a storehouse for the chromatin as soon as it is formed, for only in rare instances is chromatin ever found in detectable quantities outside of the nucleus. This is an important point to remember in the effort to understand the rôle the nucleus plays in cell life and in heredity.

When the cell has attained a certain size and its nucleus contains a correspondingly increased quantity of chromatin, both undergo division into two new cells, each with its own nucleus. In this the original nucleus manifests changes in its structure and appearance, changes which are grouped under the term *mitosis*, derived from the Greek word *μῖτος* a thread, and so named because the chromatin in the early stage of nuclear division is gathered in the form of extended loops of thread ranged parallel to each under the membrane and between two opposite poles, like the lines of longitude on a globe representing our planet. These loops then divide in the line of the equator, the membrane now disappears, the U-shaped parts, called *chromosomes*, resulting from the division of the loops, are transported to the equatorial plane, and there, or even before, each splits longitudinally, giving rise to two apparently equal daughter chromosomes. These latter are then borne to a point beyond the two poles of the original nucleus. Not only is there an equal number of chromosomes at each pole, but of every two chromosomes resulting from the longitudinal splitting of each original chromosome, one goes to each pole, which becomes the centre for a new nucleus. The chromosomes then lose their individual character and fuse in such

a way that they ultimately give a mass of vesiculated chromatin like that usually prevalent in the ordinary nucleus. This mass becomes enveloped in a new membrane, and then the cell body itself divides, giving two daughter cells each with its nucleus.

Now there are many points and exceptions in the process of division which I have not touched on. I have simply dwelt on those which are essential to the development of my subject on this occasion. I would direct attention specially to the chromatin and its history in the nucleus, to the properties of the nuclear membrane, and to the functions of the delicate cell membrane to which I have already referred.

From all the observations which have been made on the chromatin and the chromosome, and on their history and function, there can be no doubt of their vast importance in the life of the cell. The chromatin inside the nucleus guides the life of the cell and even makes its continuance possible. Gruber has shown that if unicellular organisms, such as infusoriæ, are divided by the knife, only those parts survive and reproduce themselves, which contain at least a portion of the nucleus. Parts which contain no trace of a nucleus may survive for a few hours, but they ultimately disintegrate. This makes it quite clear that the chromatin, the essential part of the nucleus, is of supreme importance to the life of the cell. How it affects this is at present unknown, but although it is a product of the action of the cytoplasm outside the nucleus, it may in turn give off continuously a substance or substances which act as stimulants or hormones, inciting and developing the synthetic or anabolic processes of the cell, as a consequence of which the cell is active and assimilative. This, I think, is the only explanation of the rôle that the nucleus plays in maintaining the continuity of the life of the cell.

Then there is the rôle of heredity. In this the chromatin plays the only part. It is now established, as a result of years of observation on the fertilization of ova in animals, that the essential element transferred from the male to the female cell is chromatin. In the transformation of the male cells into spermatozoa the chromatin is enclosed almost alone in the head, which is but a modified nucleus, and when this penetrates the ovum its chromatin goes to unite with a portion of the chromatin of the ovum to form a new nucleus which starts the development of the ovum. The new organism thus arising manifests in its development the characters of both parents. The chromatin transferred through the head of the spermatozoon to the ovum carries to the latter the qualities of the male parent. Boveri succeeded in fertilizing the ovum of an echinoderm, which had been deprived of



its nucleus, with the spermatogenic element from another species. The cell body of the new organism thus came from the female parent, the chromatin of the nucleus from the male. The characters which the new organism presented on development were those of the species to which the male parent belonged. This fact and that already mentioned, namely, that in fertilization as ordinarily observed the only material transferred from the male to the female cell is chromatin, make it abundantly manifest that the chromatin is the heredity-controlling substance.

It would appear that it is the chromatin which determines the sex of the offspring. McClung discovered that in certain insects there are two kinds of spermatozoa differing chiefly in the quantity of chromatin in the head. Fertilization with the spermatozoa of one kind, he was inclined to believe, gave rise to males only, while fertilization with the other gave female offspring only. These observations have been confirmed by the investigations of others. In certain of the arachnids and myriapods two kinds of spermatozoa are also present. More recently, also, Boveri and Gulick determined that in the nematode worm, *Heterakis*, there are also two kinds of spermatozoa, one of which furnishes to the impregnated ovum five chromosomes, the other, four. The developed ovum resulting from fertilization with the spermatozoon yielding four chromosomes is always male, that due to fertilization with the other is always female.

One might multiply instances to prove that the chromatin is the substance in the sexual cells which determines the character of the offspring. What I have advanced here will suffice to show also that the chromatin directs the life of the cell and is responsible for all its specific manifestations.

One can attribute the different manifestations to differences in composition in the chromatin of different kinds of cells. There is no escape from this conclusion. Five years ago Bardeen exposed the fresh spermatozoa of a toad to the action of x-rays for a period of from ten to thirty minutes and found that the longer exposure killed the organisms, while the shorter so affected them that, when they were used to fertilize ova from the same species, the larvæ developing from these were found to lack one cerebral lobe, one lung, one kidney, or some other structure of the normal form. More recently, Oscar Hertwig, by exposing echinoderm sperm and ova, and also the sperm and ova of frogs, to the action of radium for a time, obtained results which are almost as striking. The spermatozoa after a certain length of exposure were not killed. Indeed, in all such cases, they manifested as vigorous and active a movement as those which had not been subjected to the

action of radium, and yet, when these were employed to fertilize normal ova, larvæ did not develop, or, if development took place, it progressed only to an early stage in which the larvæ were found to exhibit either defect of structure or a great want of vitality. In the frog larvæ so produced both kinds of phenomena were observed in the same examples. Echinoderm larvæ which reached the pluteus stage were inactive on the floor of the aquarium, while the normally produced larvæ swam freely and vigorously in the upper layers of the water.

The explanation for all this is not far to seek. The chromatin owes its heredity-controlling property to its own chemical constitution, which, as already pointed out, must be exceedingly complicated, so much so that millions of isomers of it, differing only very slightly from each other, may be present in the same nucleus or spermatid head. It is, it must also be held, the number and character of these differences that determine the inheritance of parental characters by the offspring. The molecules, simply because they are complicated, are less stable than if they were simple in constitution, and on this account are easily affected by emanations from radium or by rays from the cathode terminal, just as sensitive salts of silver in the photographic plate are affected by light. Changes, great or small, in the constitution of the molecules result, and, in consequence, there may be a failure of development or, if larvæ are obtained, they exhibit more or less profound defects of structure.

It is manifest, then, that heredity, as a controlling force in the maintenance of a type and in the transmission from generation to generation of special characters and qualities, is in the last resort dependent on the complex chemical constitution of the molecules of chromatin. This being so it may be asked how it falls out that with so labile an element as chromatin there is so little change in its molecules under ordinary conditions. The answer to this question is that the physical and chemical constitution of the cell, as well as its structure, is of such a character as to reduce the possibility of such a change to a minimum. Changes do occur, however, which no structural, physical, or other constitution of the cell can prevent; for a complex, chromatin molecule may alter in composition simply from its own complexity and unwieldiness just as a huge and complicated house of cards may collapse in whole or part more readily than a simpler one. It is, indeed, to such spontaneous changes in the chromatin of the germ cells that we must attribute the variations which a species may exhibit.

In order to understand how the physical and structural characters of the cell reduce the tendency of the chromatin to vary, it is necessary to consider here the properties of the membrane surrounding the nucleus.

It has been found, as a result of a large number of observations, that the normal nucleus contains no carbohydrates or fats, and that, besides chromatin, there is present a simpler protein which serves as a stroma or framework for the nuclear cavity. There are, further, in the normal nucleus, no inorganic salts, either free, combined, or absorbed, it matters not how abundant these may be in the cytoplasm without the nucleus.

We have, then, in the nucleus a structure that does not know the inorganic world and whose contents consist of a complicated nucleoprotein derived from cytoplasm, where it is produced, and of a protein which may possibly also be of external origin. This, associated with the fact that all the chromatin in a cell is usually contained in the nucleus, is significant. Outside of the nucleus all is activity, synthesis, and change. Within there is stability and uniformity in all but the amount of the chromatin, for this is ever varying with the cyclic activity of the cell. The nucleus is the storehouse for the cell, holding and protecting its contents from changes which would be inevitable if there were free communication between the cytoplasm and nucleus.

This protective action is the property of the nuclear membrane. It allows the passage in either direction of chromatin, but of nothing else. During the life of the cell, except for the short period when nuclear division is proceeding, this membrane persists and performs its functions in this way. The nuclear membrane, then, is a structure, one of whose functions is to protect the heredity-controlling substance from change. The composition of this membrane is not in all species of cells the same. The nuclear membrane of the hepatic cell is not exactly the same as that of a pancreatic cell or of a germ cell, and it is perhaps because of this that the chromatin contained in a hepatic nucleus is different from that which is found in the nucleus of a pancreatic cell. Each species of nuclear membrane allows only chromatin of a certain type, out of all that may diffuse about it, to pass within. In consequence, a liver cell, however it may develop and divide, gives rise only to other liver cells; a pancreatic cell, only to pancreatic cells, and an embryonic germ cell, only to germ cells. We may suppose that the nuclear membranes of the germ cells act as a filter for the chromatin of a germinal type, which may be formed in all parts of the body; for it is difficult to believe that the germ cells which are affected in their nutrition by the body tissues and fluids do not receive from the other cells contributions of their chromatin. Whether the nuclear membrane may allow such chromatin to enter will doubtless depend on the character of the membrane. A slight alteration in that character should permit of variations in the offspring.

In Darwin's theory of pangenesis the body gives off from all its parts gemmules which, collected in the germ cells, reproduce in the offspring the characters of the parents, whether of inherited or immediately acquired origin. A germ cell, according to this theory, is but a microcosm of the individual organism producing it. The theory was advanced to explain not only heredity but what was accepted almost universally forty years ago; namely, the transmission from parents to offspring of such alterations in characters as mutilations may effect. The possibility of such transmission is now accepted by very few thinkers, and in consequence the theory has been abandoned on all sides, while Weismann's doctrine of the continuity of the germ plasma has taken its place. According to this the substance in the germ cells, which serves as the basis of heredity, is handed down from generation to generation unaffected by the history of the individual parental organisms, but subject to an inherent tendency to develop differences of composition within narrow limits, which on accumulation in a species would account for the formation of new characters. According to Weismann, the germ cells, though forming part of the body, never receive any material from the latter that influence their subsequent history, while the theory of pangenesis postulates that the material in the germ cells which determine the character of the offspring is derived from all parts of the parental organism. Put thus,—for the last twenty-seven years they have been so contrasted,—the two theories are mutually exclusive. On the explanation which I have given of the origin of the germinal chromatin and its filtration by the nuclear membrane, are these theories so opposed and mutually contradictory? Does it not appear more rational to regard the truth as divided between them?

I must, however, leave the theoretic side of this question and pass on to what is of immediate moment.

The nuclear membrane then has functions of extraordinary significance in the life of the cell. Further, mitosis, or the process of nuclear division, is indispensable to heredity, for a cellular organism that would not make equal division of its chromatin between its daughter cells could not endow both of them with its own characters. One of the two might receive all or the greater part of the chromatin, in which case the other, lacking in some of the heredity-controlling substance, would be a degenerate and be exterminated in the struggle for existence. Division in such a case would be merely a waste of material, not an effort at reproduction. The processes of mitosis and the physical properties of the nuclear membrane are consequently of vast importance in heredity; and in order to give due emphasis to this importance I now propose to show that these foundations of heredity are of extremely ancient origin.

All life on the earth to-day originated from an organism at the beginning in the far past. How that organism itself originated we do not know. Whatever its origin, it was probably ultra-microscopic and became of microscopic size only after a long period of time. It did not then have a nucleus and the chromatin it formed was probably more or less diffused throughout its mass. Division occurred, but heredity was not developed, and only became so when in the struggle amongst a multitude of organisms whatever was of value in that struggle could be transmitted to the offspring in a species. The sexual process also had not developed. Even to-day such non-nucleated organisms as the Cyandphyceæ, the blue-green algæ, are wholly asexual in all their cycles of life. The first development must have been a mechanism by which the cell, when it divided, gave to each of the two new cells not only half of the whole cytoplasm but also, and more important, one-half, or approximately one-half, of the whole of its chromatin. For this purpose, perhaps, besides mitosis, which is now the dominant method, other methods were evolved. One of them still persists in the foraminifer *Calcituba polymorpha*. In this form the nuclei are without a membrane, and it forms only when about to divide. This results in the production not of two but of ten daughter nuclei by a process which is utterly unlike mitosis. If we read the history of this form backwards, we will recognize that the nucleus was in all cases first without a membrane.

That mitosis only gradually developed would seem to be indicated by the defects that we still see in some primitive forms. In Peridineæ we find it does not go beyond the first stage. These, which are the chief phosphorescent forms in the ocean, are amongst the oldest types of living matter persisting to-day, and are probably direct descendants of the forms which existed before there was any specialization into animal and vegetable organisms. Of a similar origin is *Euglena* in which a defective mitosis is found.

In the end the process, that of mitosis, which gave almost or complete equality in the shares of the chromatin between the two daughter nuclei, was evolved. This occurred before the unicellular organisms became distinctly animal and vegetable in their characters, for the mitosis of the vegetable cell is in all points essentially the same as that of the animal cell and, consequently, there must have been a common origin for both. About the same time the nuclear membrane began to be a persistent organ.

The development of this structure may have been furthered by the growing salinity of the sea water. When vertebrates first made their appearance the salts in the sea were probably in concentration not more than one-third what they are now, and this date was far on in



the history of life on the earth. It is probable that the earliest ocean water was merely faintly salt, but by the time the primal cell had reached the stage in which the nucleus made its appearance the saline concentration had increased, the salts were influencing the vitality and heredity of the organisms, and, in consequence, a membrane unpermeable to salts and protecting the heredity-controlling substance from change became of enormous value, and was evolved.

It would seem that the sexual process in its essential features made its appearance first at this same stage. It also is practically the same in vegetable as in animal cells, and hence its common origin. The very fact that only organisms, vegetable as well as animal, which are nucleated undergo the sexual process shows that the latter is of very ancient origin.

All this development, therefore, happened far, far back in the history of the earth, and long before multicellular organisms arose. The rocks which are grouped under the general name Huronian are the most ancient in geological history, and yet in the upper divisions of this period, that is in what is called the Pre-Cambrian, there are found remains of highly developed animal forms. Animal and vegetable cells must have attained their typical characters ages before that. The foundations of heredity were, therefore, laid in the very earliest stage of the earth's history.

How long ago that was one cannot say. As I have already stated, one hundred million years is a conservative estimate of the time during which life has existed on the globe. Joly's estimate of the age of the earth, based on the amount of sodium chloride now in the ocean and the amount annually discharged into it by the rivers, approximated ninety millions of years. That, as Dubois has pointed out, is probably much under the mark, but even this leaves the imagination in despair. We may, however, say that for scores of millions of years the organs of heredity in the cell have been performing their function. They are consequently not greatly less ancient than life itself.

Because of the steady action of these same organs, life has been handed down through the long ages. Is it surprising, in view of all this, that they should to-day, as they will in the far future, act as unerringly as unfalteringly as in the remote past? These foundations, laid so long ago, are primal, and their dislocation for a single generation would bring the history of life on earth to an abrupt end.

To suppose, therefore, that any external condition, any food-stuff or alcohol, acting for one or many generations, can affect the heredity-controlling chromatin, is to underestimate the forces that have played their part in the cell for scores of millions of years. For ages the physical

conditions in the environment of living matter have been trying and testing those very forces and their foundations, only in the end to make them firmer and firmer as time passed. Is it reasonable to believe that any external agency or condition, such as our artificial modern life provides, can in a century or two undo all that has been so slowly and laboriously established in the long past?

And yet some one may ask: Is alcoholism not closely associated with degeneracy? To that I would answer: Yes, but as a symptom or a result, and not a cause, of degeneracy. I hold that to regard it as a cause of degeneracy is about as futile as to claim that intellectual deficiency is the cause of the defectively developed brain.

Nor can racial degeneration be brought about by underfeeding any more than by overfeeding. You will hear it not infrequently stated that the economic conditions of our modern civilization are responsible for the degeneracy of type which may be found in the slum districts of densely populated cities, and that if economic conditions were improved not only would misery and poverty disappear, but degeneracy would be eliminated from the race. Misery and poverty may be alleviated in this way, but to hope that degeneracy would thus vanish is to indulge in a foolish dream. Can the economist, the philanthropist, or the statesman, or all three combined, add by thinking on their behalf one cubit to the intellectual stature of a family of mental degenerates? In the words of Sidney Smith, "You might as well try to poultice away the humps of a camel."

The error on this point has arisen from confusion of ideas. Underfeeding, overcrowding, and other unhygienic conditions do affect the physique of the individual in a marked degree, and if he is subject to such conditions during the years of his development he reaches maturity more or less stunted and with correspondingly deficient physical, and perhaps also mental, powers. He in popular estimation would be classed as a degenerate. His children brought up under the same conditions and with the same results are similarly classed. If now their children are reared under healthier conditions and with a sufficiency of food they may—I do not say they will—be normal in physique and mentally vigorous. Here it would consequently appear to follow that the improvement in the surroundings and a sufficiency of food have eliminated degeneracy from a stirps, but the mistake made is to suppose that the individual of the first instance or the children of the next generation were degenerates. They are no more examples of degeneration than are individuals who are afflicted with chronic tuberculosis.

Genuine degeneracy is more fundamental than this. Sufficiency of food and improved hygienic conditions can no more counteract it

than they can convert a member of the black race into a white man. It is the heredity-controlling chromatin that is at fault in degeneracy, and this result is an effect of the tendency to spontaneous change in the chromatin. This tendency is responsible for more than this, for it is the factor in the production of new and unusual characters distinctive of what, in biological language, are called "sports." A newly developed character of this type may not represent a desired improvement, but the reverse and the individual in which it appears may thus be far below the type from which it arose. On the other hand, the sport may represent a greatly improved variety, and thus be a progressive form while the other would be a degenerate. So in the human species the alteration of the germ chromatin due to this tendency to spontaneous change may result in the production of a degenerate or of an individual of much more than average capacity.

This variability of the germinal chromatin is, as I have already pointed out, of two sources. In one case, the altered chromatin may be derived from without, through a slight alteration in the nuclear membrane permitting it to diffuse through into the nuclear cavity. In the other case, it arises from the labile character of huge molecules which are apt of themselves to disintegrate or change in their constitution ever so slightly, and consequently minute changes may occur early in the chromatin of the sexual cells.

These two kinds of variability are such that no structure or organ-determining heredity could prevent. It is well, however, that it is so; for if variability were excluded there would be no progress. The first unicellular organisms, if endowed with rigidly and unerringly working functions of heredity, would never have given rise to higher and more complicated types, and to-day after millions of years the only life on our globe would be that of some one simple protobiont form. There is in variability, therefore, a factor which has made for evolutionary progress. The organs of heredity have safeguarded what of value has been won, and have excluded change due to the action of external forces. The only variations which occur are those which no arrangement of a structural, physical, or chemical character could prevent.

On this view, degeneracy of type must be regarded as always dogging the heels of evolutionary progress, but it has never hitherto retarded it, for the conditions of existence which have prevailed in the past have eliminated the unfit. In the human species also these same conditions, combined with disease, have operated in the past in the same way. To-day the spirit of humanity, aided by science, has largely checked the operation of these factors, and the result is likely to be of the gravest import to the race. What effective remedy for this there may be, I do



not know. That suggested by Sir Francis Galton can only be palliative. A religion that has as one of its special tenets the restriction of the reproduction of the unfit may have its effect eventually. In the eleventh and twelfth centuries the monasteries of Europe were filled from the ranks of the ablest and best of the race, and this, in Hallam's opinion, lowered the average mental capacity of the later Middle Ages and led to a prolongation of the barbarism of the earlier. It may, in the future, be possible to use the force of religion to segregate, not the best, but the unfit, in celibate communities, and thus raise the average mental capacity of the race. At present, however, this would appear only to be a dream.

Though we may not hope for the present, we need not despair for the future. To take the first step towards a solution of a problem, we must diagnose accurately the cause of the morbid condition. To do that we must also recognize what a force heredity is, of how ancient an origin it must be, and how it may, if at all, be influenced. To know, and recognize clearly, these things will at least save time and prevent the application of quack remedies to a situation the right treatment of which will always call for the most expert knowledge and the exercise of the most thoughtful foresight of the state. Therein lies the high duty of physiology to humanity.

## ON RENAL ACTIVITY: A NEW VIEW OF THE FUNCTION OF THE GLOMERULUS

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**A**LTHOUGH of recent years a considerable amount of attention has been paid to the processes taking place in the kidney during the secretion of urine, opinion is still divided on the question of the mode of action of the glomerulus. It is universally felt that some simple hydrodynamic or physical force must be brought into play in the glomerulus, the correct interpretation of which will explain the very characteristic, anatomical features this structure possesses. Consequently the theory originally proposed by Ludwig, that the glomerulus was merely a minute filter, still receives a great deal of support. The interpretations given to experimental results have, however, been pushed to their greatest extent in order that they may be made to fall into accord with this theory. Before we can hope to make much further progress in our knowledge of renal activity, it is most essential that we should decide once and for all what is the actual meaning of the structure of the glomerulus, and what is the nature of the work it performs.

As regards the latter point, all workers are agreed that the main bulk of the water secreted in the urine is discharged by the glomerulus. The point, however, which is not so easy to settle is whether the fluid separated from the blood at the glomerular surface does or does not possess a saline concentration similar to that of the blood. Ludwig's theory necessarily assumes this, but as our knowledge has grown, more and more doubt has been thrown upon it, and at the present day there is, in my opinion, abundance of evidence proving quite conclusively that the fluid appearing at the glomerular surface may, under many conditions, be far less concentrated in salts than is the blood plasma. Hence, it follows that that fluid cannot have been filtered through the glomerular surface by the small hydrostatic pressure—the blood pressure—available within the glomerular loops. Consequently we are driven to the conclusion that the cells discharging this fluid must perform work in liberating it or, as we may express it in more concise, though less definite, terms, the glomerulus secretes. If, then, the idea of the glomerulus being a filter must be abandoned, we are again face to face with the problem: what is the meaning of the very character-

istic and peculiar structure? I believe the following conception gives the correct explanation of the glomerular function.

My view is that the glomerulus is, in reality, a means by which there may be set up at the commencement of the tubule a pressure-head sufficient in magnitude to drive the fluid down the tubule. The renal tubule is a very long structure possessing an extremely narrow lumen. If, then, we apply Poisseuille's law governing the flow of fluid along capillary tubes to the kidney tubule, we at once see that a very considerable pressure is required to drive even a small volume of fluid along such a tubule. As we have seen, it is a well-established fact that the watery part of the urine arises mainly, even under certain circumstances, entirely at the glomerular surface, and this fluid is discharged down the tubule. If we could measure the length of the tubule, the diameter of its lumen in its various sections, and then determine the volume of urine flowing along the tubule in a given time, we could, assuming that Poisseuille's law holds for the tubule, determine how great a pressure is required to maintain such a flow. To test the view, I made some experiments of the following type.

A vigorous diuresis was set up in an anæsthetized dog by the injection of a warm, saline solution. The rate at which the urine was discharged from one kidney at the height of this diuresis was then determined, and immediately the renal vessels and ureter were ligatured and the intact organ which thus contained, as far as possible, the whole of its blood and urine at the instant of ligature, was fixed in formalin. After hardening, it was cut into portions, the medulla separated from the cortex, and pieces of the cortex weighed, imbedded, and cut into sections. These were all mounted in series, and then the total number of glomeruli determined by counting. The number of glomeruli in the whole kidney was then calculated from the ratio of the weight of the whole cortex to that of the small piece examined. As there is but one glomerulus for one tubule, we thus learn the total number of tubules in the dog's kidney. In my experiments it averaged 210,000. The lumen of the tubule in its various parts was next measured. We already possessed measurements of the approximate lengths of the various sections of the tubules. With these data we can calculate the pressure-head which must exist within Bowman's capsules.

Taking one experiment as typical, I found that a pressure of as much as 93 mm. of mercury was required to drive the fluid along one of the tubules at the rate observed during the experiment.

The question at once arises: Whence is this pressure-head derived? There are only two possible sources; namely, (1) a secretory pressure set up by the cells in some part of the tubule, presumably at the

glomerulus, since the secretion of water is effected there, or, (2) a direct pressure transmitted through the glomerular surface from the blood-pressure. All attempts made to ascertain whether any of the cells of the renal tubule can set up a secretory pressure analogous to that observed in the case of the sub-maxillary gland, have completely failed. The maximum pressure at which urine can be discharged from the kidney is always some 30-40 mm. hg. less than the arterial blood-pressure. Hence my suggestion is that the pressure-head is derived from the capillary blood-pressure within the glomerulus, that pressure being transmitted in undiminished amount through the glomerular capillary wall and epithelium;—in undiminished amount, because I believe the glomerular epithelium offers no resistance, or at most, but a minute resistance to distension.

To express this view of the hydrodynamic action of the glomerulus, I term the glomerulus a "propulsor."

The recognition of the fact that a very considerable pressure-head is absolutely necessary to drive the fluid from the glomerulus through the tubule, once and for all disposes of any possibility of filtration as an effective process at the glomerulus surface. For, after we have subtracted the necessary pressure-head, nothing is left wherewith to effect filtration. When regarding filtration as the active process at the glomerulus, it is commonly assumed that the glomerular capillary pressure is approximately that of the aortic pressure. This, however, is certainly incorrect. The glomerular capillary pressure is undoubtedly, in many conditions, much higher than an ordinary capillary pressure, such as we find in most of the systematic capillaries, because the afferent vessels to the glomeruli are larger than most arterioles; but still they are small, and the rate of blood-flow through them is so considerable, that a good deal of the energy of the blood must be used up in overcoming the resistance they offer. I estimate it at a loss of pressure-head of about 30 mm. hg., which, with an aortic blood pressure averaging about 120 mm., leaves a glomerular capillary pressure of about 90 mm. hg.

Many of the points which have been adduced in support of the filtration hypothesis are still better interpreted upon this propulsor view. It explains, for instance, the general structure of the glomerulus; namely, that of a series of thin-walled, distensible loops projecting into the interior of a capsule, which latter is not free to expand beyond a certain limit. In the next place, the fact that the afferent vessel is of large diameter is for the purpose of causing but a relatively small loss of pressure-head between the renal artery and the interior of the glomerulus. The fact that the efferent vessel is smaller than the

afferent, thus offering a heightened resistance to the blood-flow at this point, enables us to realize that there is here a rapid loss of pressure-head, so that the pressure in the capillaries around the tubules is much lower than the glomerular capillary pressure. Moreover, this arrangement permits of an expansion of the glomerulus without producing any appreciable loss of pressure within the glomerulus. The propulsor view enables us to explain yet another feature in the anatomy of the kidney; namely, that the organ is surrounded by an inextensible capsule. This is to prevent a dangerous over-distension of any of the more fragile parts, particularly the glomerular loops and Bowman's capsule.

An important point in confirmation of my view is the fact that the glomerulus is by no means a rigid structure. On the filtration theory, either the glomerulus must possess rigid walls or the loops must be continually pressed against the inner surface of the capsule. This latter condition is by no means always the case, as was proved by the direct microscopic observations of Nussbaum upon the kidney of the newt with the circulation still flowing. He describes them as pulsating, and as varying in size, at times filling up the whole capsule, and then again retracting and leaving the wall of the capsule. The appearance of the kidney after active diuresis also offers many points in favour of my view. Thus the tubules, especially the proximal tubules, are found widely dilated, and the glomerulus can be seen in most instances to have left the wall of the capsule. The dilatation of the tubule is unaccompanied by any increase in the total transverse diameter of the tubule; what does, however, take place is an increase in the total length. Both these conditions are produced by the high pressure of the fluid within the tubule, especially at its upper end. Lastly, the propulsor theory gives a complete explanation of all the observations that have been made upon the ureter pressure. Without entering into detail upon this point, I may state that the maximum ureter pressure simply measures the pressure which the glomerulus is capable of setting up within the capsule, that is, it is a measure of the glomerular capillary pressure. The flow of fluid along the tubule naturally decreases as the pressure at the outlet of the tubule rises, and ceases when it reaches the pressure at the head of the tubule.

There is one other point upon which I would take this opportunity to express my opinion. This is the mode of action of the diuretic substances of the caffeine group. These are so commonly employed as therapeutic agents, that it is especially important we should know upon what structures in the kidney tubule they act. It becomes especially important because a number of observers who have studied the kidney in this respect have drawn the conclusion that caffeine

and its allies only act upon the vascular mechanism, and hence that they are perfectly safe diuretics to give in cases of kidney disease, since they do not act upon the renal epithelium.

My reason for referring to these diuretics at this time is because I think we cannot condemn this view too strongly. Proof has accumulated in a number of directions that while caffeine does act upon the circulatory mechanisms, upon the heart, and upon the blood-vessels, it also exerts a very decided action upon the cells of the renal tubules. Histological examination of normal kidneys after an active caffeine diuresis brings out this point in the clearest possible manner. Experiments in which the gaseous metabolism of the kidney were studied still further emphasized the fact by showing that a greatly increased consumption of oxygen and formation of carbonic acid took place within the kidney when caffeine was administered. Moreover, the study of the chemical composition of urine secreted under the influence of caffeine has shown that under its influence the renal epithelium loses the power of holding back chlorine within the blood after the chlorine content of the blood has fallen to its normal level; whereas a normal kidney at once ceases to excrete chlorine when this occurs. A kidney under the influence of caffeine will, on the other hand, continue to discharge chlorides.

Lastly, experiments upon the frog's kidney, in which it is possible to eliminate the glomeruli and leave the tubules only in activity, have conclusively proved that caffeine can make the tubule cells secrete. There is no doubt whatever, then, that caffeine and other bodies of the purine group are specific stimulants to the cells of the convoluted tubules. In addition, we know that they act upon the renal blood vessels and that, moreover, they do excite a more copious flow of water from the glomerulus. But the point I wish to emphasize is the caution that, in administering caffeine to a patient suffering from nephritis, we must be sure to recognize that the drug acts powerfully upon the tubular epithelium.



## RHEUMATISM AND SO-CALLED CHRONIC RHEUMATISM

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THIS is a commonplace subject in one aspect; an involved one in another. In the list of complaints made by patients, "rheumatism" would take a high place, and in the list of diagnoses made by physicians, it would also be well up in the list. Patients come to us complaining of it, but, whether we are justified in letting them depart with such a diagnosis is a question for discussion. Certainly it is a diagnosis which is usually satisfying to the patient, but whether it should be satisfactory to the physician is another matter. In this faculty of satisfying the patient it shares the honour with "too much uric acid" and "something wrong with the liver." For the southern states we might add, "a touch of malaria." However, such a diagnosis is about the same as that said to have been given by Sir William Gull to the insistent wife of a patient when no certain opinion could be given: "Madam, your husband has a cachexia."

It is particularly to the use of the terms "rheumatism" and "chronic rheumatism" that attention is here directed. To what is the term "rheumatism" applied? If enquiry is made into this question, we are struck at once by the fact that it is employed in various ways. Thus, it is used to designate the disease known as rheumatic fever and also as a synonym for arthritis, as, for example, gonorrhœal "rheumatism." Now it is very evident that a term cannot be employed to designate the whole and a part of the whole with any clearness of meaning. We cannot well use it to designate any form of arthritis and also a particular disease often characterized by a special form of arthritis. A recent example is found in a journal<sup>1</sup> to which we look as a guide in matters of correct usage; namely, the heading "Tuberculous articular rheumatism." Now, if the word "rheumatism" is here used as synonymous with arthritis, we have "tuberculous articular arthritis"; which is absurd. If it is used as meaning rheumatic fever, surely no one believes that we have a tuberculous rheumatic fever. Actually, the article was on tuberculous arthritis. Again, an article in one of the English

Read before the Montreal Medico-Chirurgical Society, October 7th, 1910.

<sup>1</sup> *British Medical Journal*, August 20th, 1910. This is in the epitome (page 29), and is a translation of the original, but it serves equally well for an example.

journals was headed, "Scarlatinal rheumatism." Can any one tell from this title what is meant? It may be intended to represent a hybrid combination of scarlet fever and rheumatic fever (like the typho-malarial fever of the South), or arthritis in scarlet fever, or a particular kind of rheumatism. Then again, the term rheumatism is applied to many other conditions which have nothing to do with arthritis; for example, vague pains are often so designated. Some of these conditions will be taken up more in detail later.

That such a state of affairs in unsatisfactory, requires no argument. To have an accurate knowledge of any subject requires a definite terminology. Language is our main method of communication, and if six men are using the same term to mean six different things, there can be no satisfactory discussion of such a subject until all are agreed upon what is meant. To know how many diseases have been given the diagnosis of "rheumatism" would be a sad commentary on our powers of diagnosis. It is certainly a sort of arthritic scrap basket into which many guesses are thrown, and it cannot be described as a term which is used with any definite meaning.

Then again, give a name to a condition and you have to a considerable extent settled it as regards your mental attitude, especially concerning any probability of further enquiry. A patient comes with some obscure condition—arthritic or otherwise—which is accompanied by pain, and you say that he has "chronic rheumatism." Now this term has no generally accepted meaning, and by giving it as a diagnosis the problem is, so to speak, pigeon-holed, and that once done it is only a very unusual mind which pulls the contents out and goes over them again. Let a man say that he does not know what the disease is and there is hope that he will endeavour to go farther and try to discover what is really wrong. As Gowers has said, in reference to the loose use of the word hysteria: "A conception which conceals whatever it covers; we must rescue it from whatever we wish to study; it can be put back again afterward if desired."

The influence of the use of the terms rheumatism and chronic rheumatism is also important in our attitude towards the whole subject of arthritis. In a very confused state at the best, the difficulty of arranging our knowledge of it is immensely added to by the loose use of these terms. Any one who has tried to read French or German articles on the subject of arthritis will appreciate this. It almost seems as if the uncertain meaning which might be conveyed by the term "rheum" has been inherited by its offspring.

It is, perhaps, more instructive to study in detail some of the conditions to which the designation "chronic rheumatism" is applied.



The number of these is proof of the entire absence of any agreement as to what the designation means and suggests how many diseased conditions are covered by its mantle.

1. Is there a condition following rheumatic fever, in which there are permanent changes in the joints? If this does occur, to it the term "chronic rheumatism" might be properly applied,—if by rheumatism we mean rheumatic fever. However, if there is any evidence, that an attack of rheumatic fever leaves permanent changes in its train, the writer does not know of it. On the contrary, one of the most characteristic features of the arthritis of rheumatic fever is that the joints are left undamaged. The writer considers it a good rule that, if there are permanent changes left in a joint after an attack which has been regarded as rheumatic fever, it is well to go back and revise the diagnosis. This view is opposed by some who consider that rheumatic fever may leave permanent arthritic changes. However, the writer has never seen such a case, although on the watch for it for many years. There are cases of rheumatic fever in which cardiac complications persist, and to these the term "chronic rheumatism" might be applied, but it is practically never used in this sense.

2. The various changes which follow infection with certain organisms, such as the gonococcus, tubercle bacillus, etc., are often termed "chronic rheumatism," or are qualified by the causal factor, e. g., "gonorrhœal rheumatism." This may be regarded as showing rather the careless use of the word than an essential mistake in diagnosis, for the man who speaks of "tuberculous rheumatism" probably refers to an arthritis due to the tubercle bacillus and not to a combination of tuberculosis and rheumatic fever. However, the use of the word "rheumatism" is likely to suggest an association of some mysterious kind with a "rheumatic" process.

3. The various forms of arthritis which occur secondarily to various infections without any evidence of the presence of organisms in the joints. These are comparatively common and are important to recognize. Take, for instance, the arthritis which may occur with, or follow, an attack of tonsilitis. Here the original, local infection is usually due to streptococci, but these are not to be found in the joints, and the most probable explanation is that the arthritis is due to toxins. It is not easy to mark this group by any sharp boundaries. Such attacks of arthritis may subside and leave no damage behind, or some changes may be left, which subsequent attacks may increase. In this latter event, the cases approach very closely to those usually termed "arthritis deformans."

4. Gout. This is more common than is usually considered, and

is often termed "chronic rheumatism." There is an impression that gout is a very rare disease in this country, but this seems to be erroneous, and the incidence is probably not much below that in England. It is well to rid the mind of the idea that gout follows only upon a course of high living in the individual or his ancestors. This is quite incorrect, for while, of course, it may be inherited, yet, in this country, beer drinking seems to be the most important single cause. In the recognition of gout it has to be kept in mind that the classical picture is not invariably present and that polyarthritis occurs frequently.

5. In any discussion on chronic arthritis, the form which in this country usually goes under the name of "arthritis deformans" deserves attention. It is much commoner than is usually supposed, carries the probability of crippling the patient more or less, and too often dooms him to years of suffering. The chance of aiding these patients is lessened by the idea that the condition is "chronic rheumatism" or "rheumatic" in character, and so requires treatment adapted for a "rheumatic state," which is usually the worst that could be chosen. As an example of this, we often find that meat is omitted from the diet and the patients are dosed with salicylates for weeks and months. There is no evidence that arthritis deformans has any association with, or relation to, rheumatic fever.

Reference may be made to the term "rheumatic gout," which is sometimes applied to arthritis deformans. This should no longer be used, as the disease has no relationship to rheumatic fever or gout. The same may be said of the term "rheumatoid arthritis," which is used by many to designate one of the forms of arthritis deformans. As all our teaching is devoted to showing that it is *unlike* rheumatic fever, the use of a term suggesting its likeness to that disease seems unfortunate. We have to plead to a similar usage in the term typhoid fever, but the use of the term "typhosus" to describe the causal bacillus renders any change undesirable. Besides, there are few places where typhoid and typhus fevers can be mistaken for each other; there is no place where rheumatic fever and arthritis deformans may not be the cause of difficulty in diagnosis.

That there are difficulties in regard to many points associated with arthritis deformans we all know. It is, therefore, the more important to lessen them as much as possible; and so let us do away with the confusion caused by applying the term "chronic rheumatism" to it. "Cover a thing with a name and you conceal it;" we need all the light we can get on arthritis deformans.

6. There are certain conditions of the spine—some of which belong to arthritis deformans—and sacro-iliac joint which are too apt

to be dismissed with the diagnosis of "chronic rheumatism." How many patients with spondylitis or sacro-iliac joint disease have been given this diagnosis, it is hard to say. The impossibility of any clear idea of what is the cause of the symptoms and, as a consequence, the inability to give relief, are very evident if we are content to label the symptoms of such lesions as "chronic rheumatism."

7. Changes in the joints in advanced life. We are all familiar with the changes seen, not infrequently, in the joints of aged persons, especially those who have done hard, manual work. In many cases these are due to arthritis deformans, but in other instances the changes seem to be due more to the "wear and tear" of life, and are more of the nature of a degeneration than of an inflammatory character. It would be about as reasonable to term a Charcot joint "tabetic rheumatism" as to apply the word to these conditions.

8. Of conditions which have nothing to do with arthritis, it may be thought unnecessary to speak, did not experience show that they, too, frequently are disposed of by the name (chronic rheumatism) given to them. These are so various that there is no end to them, but some may be noted.

(a) There are many patients who suffer from obscure pains in the muscles and about the joints in whom evidence of any arthritis is entirely lacking. To these the term "muscular" or "chronic rheumatism" is frequently applied. As regards the former term there is no evidence—so far as known to the writer—that there is a specific involvement of the muscular tissues in connexion with rheumatic fever or any chronic muscular condition which follows it. Of course there is wasting, but that is not what concerns us at present. The condition would be better described by the term "myositis" or "fibrositis," which is probably a more accurate designation in most cases. The process is usually in the fibrous tissues and repeated acute attacks may be followed by permanent change. It is evident that so long as we give it a name which suggests a rheumatic condition, so long will we have incorrect views as to the essential nature of the process. Our therapy must then be directed to a name rather than to a disease, and this is also an important reason for trying to discard anything which gives a false conception of what we have to treat. It is always too easy to form the habit of treating the name of a disease and not the patient.

(b) Occupation neuroses. These are very common and may be seen in motor men, those who run machines which involve special positions or demand special work of one leg or arm, those who sit in constrained positions—sometimes seen in locomotive engineers, and

those in many trades. It is easy for the physician to call them "chronic rheumatism," but hard for the patient to be helped if he does.

(c) Flat foot. This may give symptoms which are not recognized as due to the local condition. Flat foot is comparatively common, and, unless a careful examination be made, is often overlooked. The absurdity of treating such a condition by remedies which are given for rheumatism is very evident.

(d) Neuralgia and neuritis. Minor grades of both these conditions are easily dismissed by calling them "rheumatism." We know how difficult it often is to be sure of such conditions without a very thorough examination. Give them a name and it is easy to be satisfied with that.

(e) The pains due to varicose veins are sometimes regarded as being due to "chronic rheumatism." The pains of *tabes dorsalis* have also received the same diagnosis. Not infrequently the pains due to syphilis have been regarded as rheumatic (quite apart from syphilitic arthritis). Sarcoma has had the same term given to it. In tuberculosis, too, valuable time has been lost and serious harm done by regarding the pains as due to "rheumatism." Patients with hip-joint disease have gone under this designation until the progress demanded another diagnosis.

(f) A great variety of examples might be cited, but I must be content with two additional ones. One was in a patient who had carcinoma of the spine, the pains of which were regarded for some time as due to "chronic rheumatism." In this case no harm was done as regards the outlook for the patient. The second instance was in a man who complained of pain in one leg, and limped a little in walking. He volunteered the diagnosis of "chronic rheumatism," which he said had been made by several physicians, none of whom had helped him. The reason was very evident, after one glance at his leg—it required some persuasion to get him to undress: "No other doctor bothered about having my clothes off"—he had a *popliteal aneurism*.

It is evident that the conditions grouped together under the designation "chronic rheumatism," are most diverse in their essential nature and are for the most part characterized by pain. This is surely indefinite enough if the term "rheumatism" is applied only to conditions associated with some form of arthritis, but it is, unfortunately, given as a diagnosis for painful conditions which have nothing whatever to do with the joints. One word may be said about the so-called therapeutic test. In the minds of many the opinion is held that a pain which is relieved by any of the salicylate preparations is necessarily "rheumatic." On the contrary, we know that pain from many other causes

is relieved by these drugs. The influence of the weather is also given prominence by many. Because certain pains are worse in wet weather is no reason why they are "rheumatic." Pains associated with many conditions, such as old injuries, occupation, neuroses, neurasthenia, etc., are often rendered worse in bad weather. We do not necessarily, therefore, consider them "rheumatic."

A necessary association of this carelessness in diagnosis is carelessness in proper examination. If one has the habit of using the designation "chronic rheumatism" as a diagnostic hold-all, it becomes too easy to use it and be content with such a loose term. Give a thing a name and you have, to some extent, disposed of it. Had the physicians who saw—I was about to say examined, but that would be incorrect—the patient with popliteal aneurism, not had a term like "chronic rheumatism" to give in explanation of the cause of the pain, it is reasonable to suppose that they might have taken the trouble to examine the patient. If we were content with the diagnosis of "fever" in every patient who had an elevation of temperature, think what a chaotic state of affairs we would have as regards any conception of what particular fever we were dealing with.

It may be thought that the possibilities of error are discussed at undue length, but it is well to recognize how many of these there are, and how many various diseased conditions may be put under one name. So long as one is content to use a diagnostic term which means nothing, so long is progress impossible. As long as men were content to designate malarial, typhoid, typhus, and relapsing fever under the heading of "continued fever," so long was any differentiation difficult. Exactly the same applies to the forms of arthritis. The use of proper terms is essential to clear ideas about what the terms stand for. To have different men using the same term to designate various conditions is worse than if each spoke a different language and knew only his own. Then each certainly could not know what the other said, but would be fully aware of his own ignorance. But, if two men use the same word with different meanings, each thinks he understands what the other means and is doubly deceived. Language does more than conceal the thought. The writer remembers well a discussion he had with a physician in Paris who had written extensively on arthritis. In some of his articles it had been impossible to know exactly what was meant by certain terms. But verbal explanations gave little help, for it was necessary at every use of the word "rheumatism" to stop and inquire what was meant. At one moment it meant any form of arthritis, at another some special form; e. g., tuberculous arthritis and again rheumatic fever.



Thus far it is evident that there has been little but destructive criticism, and such has been the intention. We may now turn to the question of construction, and discuss any suggestions as to the possibility of improvement.

1. When we are using a term which is employed in various ways and has no definite, generally recognized meaning, we must either drop it altogether or accurately define its meaning and adhere to that. Which is the easier here? In the writer's opinion the first is the better, and we should gain by giving up entirely the use of the terms "rheumatism" and "chronic rheumatism." It may be objected that we employ these terms a great deal, but it is evident that the more we employ them, the greater the confusion. If the designation carries no accurate meaning is its retention any advantage? On the contrary, it is not a distinct disadvantage in every way? Use the term "rheumatism" as equivalent to arthritis if you will, but then it is not proper to use it for rheumatic fever; the difficulties connected with this are evident. For the disease known as acute inflammatory rheumatism or acute rheumatic fever, the designation "rheumatic fever" might be employed. The adjective acute seems to be unnecessary. The designation, acute articular rheumatism, is not a good one. As well speak of it as acute endocardial rheumatism; in many ways it might be better if we did. Certainly the endocarditis is of much more importance than the arthritis and in children occurs more frequently.

The objection may be made that it is not easy to teach an old dog new tricks; that when one has spoken of "gonorrhœal rheumatism" for many years it is not easy to speak of "gonorrhœal arthritis." This objection is a natural one, but if the change is worth adopting, of course there will be some difficulty. Certainly every one who wishes to have clear ideas—or as clear as our knowledge permits—on the whole subject of arthritis will find it a gain to drop the word "rheumatism" from his vocabulary. If we started our students with a proper terminology, it would not be long before the profession would have it as a matter of course.

As regards "chronic rheumatism," if by it chronic arthritis is meant, why not say so; if "chronic rheumatic fever" is intended we doubt if such a thing exists. It will not require long for any one by taking heed to drop the term "chronic rheumatism" from his list of diseases, with a gain both in accurate thinking and especially in accurate diagnosis. Let every one who has been in the habit of making the diagnosis of "chronic rheumatism" stop the next time he is tempted to do so and consider what he means by it and whether the designation fits the condition present in the patient. Most of us who do this conscientiously

will be surprised to find in what a slipshod way the term has been used. It may be objected that the public understand and use the term and it will be hard to re-educate them. The profession gave them the term in the first place, and if the profession leads in a reform they will soon follow. Then again, dropping the term "chronic rheumatism" would limit a most common cause of non-professional treatment. There are, probably, more home-made cures for this "disease" going about than for any other. We all know instances of injury done by well meant but harmful advice in regard to "cures for rheumatism" given by the laity. It might also help to lessen some of the foolishness about uric acid. "Chronic rheumatism" and "uric acid" seem to be two idea centres in many cerebrums which must be connected by a specially developed set of nerve fibres.

2. The attempt to carry some definite ideas as to various kinds of arthritis, so that one can classify cases on some system. With the systematic effort to properly classify our cases, it will be found that more and more are put in their proper pigeon-holes and not thrown into the scrap basket of "rheumatism."

3. More care in diagnosis, so that conditions which have no arthritic manifestations are put where they belong. This is, perhaps, the most important of all, as so many maladies called rheumatism have not even the excuse of any arthritis as part of their manifestations. Let us try to decide whether we have a form of arthritis—calling it such and not rheumatism—or neuritis, or neuralgia, or an occupation neurosis, or whatever it may be, and call it by a name which belongs to the disease.

The whole argument is that there is no entity which we can properly term "chronic rheumatism." If this be true, how difficult it is to carry out proper treatment for a malady which has no existence as a distinct disease; for without proper diagnosis we merely treat a name, and how unsatisfactory that is requires no emphasis. Those of us who teach therapeutics are being constantly reproached that our students graduate with the tendency to treat the name of the disease and not the patient. In this paper I have tried to point out the reasons for doing away with one of the names which is so often treated. To treat a name in your own mind is not going to help the disease in the patient's body. The public often reproaches us for the inability of the profession to cure "chronic rheumatism." Is it any wonder, when in so many instances the actual condition is not properly diagnosed but is given a name which does not have any reference to it, and to this the treatment is directed; witness the patient with popliteal aneurism, on whom much good medicine has been wasted. We can still echo the hundred

year old sentiment of Haygarth that a number of conditions are grouped together as rheumatism, which have no other feature in common than the occurrence of pain.

That you will agree with me entirely, I do not expect, but I ask that you consider the matter and reconsider the diagnosis of "chronic rheumatism" when next it comes to your lips. I believe that it would be a gain if the use of the term "rheumatism" could be given up. It is not necessary.

My paper, perhaps, recalls to you a quotation from "Alice in Wonderland."

"Have some wine," the March Hare said in an encouraging tone.

Alice looked all round the table, but there was nothing on it but tea. "I don't see any wine," she remarked.

"There isn't any," said the March Hare.

"Then it wasn't very civil of you to offer it," said Alice angrily.

Perhaps you will consider it not very civil of me to read a paper on what, in my opinion, does not really exist. However that may be, I hope these remarks may suggest some consideration on the subject of the next diagnosis of "chronic rheumatism."

"LE BLUFF AMERICAIN: En mars 1909, un certain médecin américain, du nom de Flexner,—Abraham, de son petit nom—est venu à Montréal dans le but d'étudier le fonctionnement des universités Laval et McGill et faire rapport au nom d'une institution philanthropique américaine désignée sous le nom de 'fondation Carnegie.' (1)

"Son rapport sur l'école de Médecine Laval, à Montréal, est très défavorable. Malheureusement pour la fondation Carnegie et aussi pour nous, à l'étranger, tout est faux dans ce rapport: chiffres, dates, salaires, enseignement théorique et pratique, etc. C'est une infamie.

"Son rapport sur McGill est très élogieux. Cela se comprend: affinités de races, belles batisses, riches dotations. L'américain, bluffeur, avant tout, se laisse facilement impressionner par ce qui frappe la vue: marbres imposants, colonnes dorées. C'est, du reste, sa seule façon à lui, d'impressionner l'étranger."—*L'Union Médicale du Canada*.

*December, 1910.*



## THE ASSOCIATION OF MALIGNANT DISEASE OF THE THYROID AND MULTIPLE LIPOMATA

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TWO years ago, whilst working in the Pathological Institute of the Freidrichshain Krankenhaus in Berlin, Dr. Goodall had occasion to assist Dr. Ellis, of Jefferson College, Philadelphia, to perform an autopsy upon a dog which he had kept under observation for some months. We wish to express our indebtedness to Dr. Ellis for his kind permission to quote this case.

The animal had a large tumour growth in the neck, and showed symptoms of dysphagia. The dog was killed by means of chloroform, and it was found to have been suffering from primary malignant disease of the thyroid gland, which was greatly enlarged and had infiltrated the surrounding tissues of the neck. The neighbouring glands were slightly involved, but most characteristic was the extensive involvement of the lungs. Both lungs were studded with hundreds of metastases, the largest of which were about the size of an almond nut. The great majority of the metastatic growths were small. On section, the lung tissue retracted about these growths, which differed in colour from the normal, pale-pink pleura and pulmonary tissues. The microscopic sections showed that the metastases were made up of thyroid tissue in a state of rapid development, and the sections of the thyroid itself showed it to be a true thyroid carcinoma.

The dog had also three large lipomata, circumscribed and encapsulated. The largest, about the size of the palm of the hand, was situated over the left shoulder. The others, smaller in size, were situated on the hip and chest. These were made up of true fat cells without any signs of malignant tissue.

The coincidence of these two diseases made a profound impression upon Dr. Goodall, and immediately upon his return from Germany he had occasion to see two patients in whom he found the same association. In July, 1909, he was called to see Mrs. W., aged sixty-two,

who had been seized with sudden, severe pain in the lower part of the abdomen. She had vomiting and diarrhoea. The daughter, whom he had treated for pelvic trouble, thinking that her mother's condition arose from the pelvic organs, called him to attend her. The case proved to be one of ptomaine poisoning. But it was found, in addition, that the patient had a huge goitre. It measured six inches transversely and extended deeply into the root of the neck under the sternum. She suffered a great deal from pain, dysphagia, and dyspnoea. The glands on both sides of the neck were enlarged and the primary growth infiltrated the sterno-mastoid muscle on the right side. There was every sign of malignancy, and the course of the disease proved the correctness of diagnosis. The other systems were normal as far as could be made out. Upon relating her symptoms, she said that with the onset of the enlargement of the neck she had developed small tumours over her body, and that they had grown slowly, but they were absolutely painless and caused her no inconvenience. Under careful examination, seven lipomata, somewhat flattened and varying in circumference from one and a quarter to three inches in diameter, were found. The centre of these was about one-half to one inch above the surrounding skin surfaces, and from this point declined slightly to fall off suddenly at their periphery. This appearance was uniform and seemed to be due to pressure of the clothes. The lipomata were chiefly on spots subject to rubbing or irritation, as on the flexor surface of the forearm, on the shoulder, and on the inner side of the thigh.

The patient lived six months after she was first visited, and died of a terminal pneumonia. Previous to death she had had absence of breath sounds over the right base of the lung, due to compression of a bronchus. Some weeks later she developed a pneumonia over this area. An autopsy could not be obtained. The new growth had extended a great deal and had become solidly fixed, and erosion of the upper part of the sternum had begun. During the six months before death, she developed two more lipomata, one on the arm and another at the back, at the level of the waist line.

In October, 1907, while attending a young woman, Dr. Goodall met her mother, Mrs. D., an elderly lady of fifty-nine years, tall and slender. She complained of weakness, cough, and dyspnoea. Upon examination he found a large tumour of the neck, which caused her a great deal of discomfort. In 1908 she was operated upon by Dr. James Bell and the thyrioid was removed. In the fall of 1909 there was a recurrence in the glands on both sides of the median line. The left was the larger, and it pushed the sternal attachment of the sterno-mastoid very far forwards. In the episternal notch a rounded mass

rose and disappeared upon deglutition. Clinically, there could be no question that there was a recurrence of malignancy. On asking if she had any tumours about the body, she quickly answered that she had, and that they were distributed chiefly upon the upper extremities and chest, both back and front. Three of them were on the forearms. We then looked up her clinical history and found that the diagnosis was carcinoma of the thyroid. The sections of the removed gland were also examined to confirm the pathological report, which had been sent by the microscopist to be filed with the history.

Another case came under our notice not long since. We are indebted to Dr. Garrow, surgeon to the Royal Victoria Hospital, for the privilege of reporting this case. A woman, fifty-five years of age, came to the hospital complaining of enlargement of the neck. She had a large thyroid, which extended widely into the tissues on either side of the growth. Only a portion of the growth was removed, as the tissues were so infiltrated that it was quite impossible to remove the whole of the tumour. The patient had five lipomata scattered over the upper extremities and chest. Sections of the portion of the growth removed from the thyroid showed it to be a very malignant, primary carcinoma of the thyroid gland. We ourselves have seen all these cases and examined the lipomata. In this and in the previous case the patients had neglected to mention these fatty tumours when relating the history of their illness, consequently they might readily be overlooked in any given case.

Since writing the foregoing, we have been told of another case of malignant disease of the thyroid, in which the physician states that there were no lipomata. The patient was a man of forty-five years of age. We have purposely omitted this case because of the fact that two years had elapsed since he was seen, and owing to the ease with which lipomata may escape the notice both of the patient and physician. We have recorded only cases that we have seen, and in some of these we were able to find large lipomata where they had been previously overlooked. In this enquiry we have recorded all the cases of malignant thyroid that we have seen. The thyroid enlargement seems in each case to be the one malady that looms up in the patient's mind, and she overlooks the painless tumours which cause her so few, if any, symptoms.

In striking contrast to these, we have examined the reports of fifty-five cases of either adenoma or so-called cystic disease of the thyroid, and in not one was there a coincidence of the thyroid affection and lipomata. Are we justified then in assuming that there is a direct interdependence between these two affections; namely, carcinoma of

the thyroid and multiple lipomata? If so, of what nature is this association? In answer to the first question, we think that the relation of cause and effect is amply justifiable. Lipoma, as such, is in itself not a very common affection, and carcinoma of the thyroid is an exceptionally uncommon disease. In the Royal Victoria Hospital there have been but two cases of malignant thyroid disease recorded in fourteen thousand cases. Moreover, being a fatal disease, one might expect to find it more frequently in the autopsy records, yet there was but one case recorded in the last fifteen hundred autopsies.

Would it not be a very singular coincidence if two so relatively uncommon diseases were always associated without the one being caused by the other? Will the law of accidental association allow of such elasticity as to include these cases? Without any hesitation, we think we must assume that the lipomata are the direct result of and caused by the perverted or arrested function of the thyroid. To substantiate this interdependence, we need but to refer to other diseases of a lipomatous nature, or, to be more exact, diseases in which fatty change is the characteristic feature, and in which thyroid insufficiency seems to enter as a cause.

In cases of obesity, a goodly number show remarkable improvement when treated with ten or fifteen grains of thyroid daily. Allbutt states that the best results with this treatment are obtained in women who have passed the menopause. In men, on the other hand, the treatment has little or no effect. James Fletcher has had some excellent results with thyroid treatment of obesity in women of middle age. Osler states that about fifty per cent. of such cases are benefited by thyroid. Rolleston writes that it is probable that, in some of these cases of obesity, there is a measure of thyroïdal inadequacy without obvious atrophy of the gland, hence the improvement noted when thyroid extract is administered. Leichtenstein, Wendelstadt, and Ewald also report very successful treatment.

Then, if we turn to Dercum's disease, which is essentially an overgrowth of adipose tissue, either in the diffuse or in the true encapsulated type, we find that thyroid gland defects enter, in no small number of the cases, as an associated or causative factor. The only treatment of this disease, that has offered encouraging results, has been the thyroid treatment, which again lends great weight to the belief that thyroid extracts and lipomatous tissue exercise some balance in the human economy, and that the disturbance of the function of the thyroid frequently leads to vagaries in the development of fat. Myxœdema and cretinism offer somewhat similar, though not parallel, changes.

A rather interesting feature in our cases is that they are all, with

one possible exception, women, and all past the menopause. This is quite in accord with the statistics of Dercum's disease. The ratio in this disease is about one male to five females. It is surprising to find how many of the cases which have developed Dercum's disease also developed an early menopause. So it would seem that women who are disposed to early menopause, and women who have passed the menopause, have a greater tendency to develop those diseases over which the thyroid exercises a controlling influence. This seems to suggest strongly a relation between ovarian function and thyroid secretion. This theory is one which is advanced for the relatively small percentage of cases of Dercum's disease in the male, inasmuch as the function of the testicle continues into very advanced old age, and the sexual organs exercise a compensatory influence with the thyroid, but that this is lost when the ovaries atrophy after menopause. That there is between the ovaries and thyroid a true interdependence either as compensation, or as cause and effect, we have been able to establish through clinical cases which will soon be published.

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THE Board of Directors of the Vancouver General Hospital are contemplating extensive improvements and enlargements of that institution during the coming year. At a meeting of the directors, held in November, plans were submitted for the new power plant, boiler house, and steam laundry, the present plant being inadequate for the future needs of the hospital. In addition, plans for a new wing were also exhibited which, when carried out, will complete the original design. An extension of the administration building was included in the work to be done, and it was estimated by the architect that it would require the sum of \$300,000 to carry out the additions and changes. Alderman Hepburn, who is a member of the board of directors, was asked to arrange for the city council to submit a by-law, authorizing the expenditure, to be voted on at the annual election.

The hospital authorities have also been considering the necessity of making further provision for the care of infectious diseases. With the contemplated additions of the surrounding municipalities to the City of Vancouver, it will be necessary to provide much more accommodation for this class of diseases. The board is now considering the matter, and at no distant date will have an announcement to make.



## EHRLICH-HATA REMEDY FOR SYPHILIS

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THE most important recent work in syphilis is to be credited to Ehrlich and his co-workers, Bertheim and Hata. The remedy which they have devised is now well known by the term "606."

A brief, historical resumé may be of interest in tracing the steps which have led to the production of this new remedy. At the outset it may be said, in order that the cabalistic designation "606" be understood, that Ehrlich had made 605 unsuccessful attempts to produce a remedy which would be active against the *treponema pallidum*; with the 606th attempt came success, hence the title "606." As is well known, Ehrlich has for many years carried on very exact experiments, which have led to the preparation of "606." Interested in stereochemistry and therapeutics, he conceived the idea of developing a so-called "chemico-therapy," by preparing various synthetic compounds, whose formulæ he could arrange, and re-arrange, by the substitution of certain radicals for those already present; such preparations, it was hoped, would be parasitotropic without being organotropic; that is to say, substances which would fix themselves in a specific fashion to the casual organism and kill it without in any way interfering with the function of the organ invaded. Ehrlich hoped in this way to obtain what he has called a "therapia sterilisans magna."

This work had taken a very definite direction since it had become known that certain protozoan infections were amenable to treatment with arsenical preparations. Since the work of Wolferstan-Thomas, Koch, Kopke and Ayres, Uhlenhuth, and Hoffmann, who showed that atoxyl, an arsenical preparation, was active against trypanosomes, and in spirilla and spirochæte infections, work in this direction has been carried on not only by Ehrlich but by various others. Salmon, in 1907, noted that atoxyl and other arsenical preparations were useful in the treatment of syphilis. It was soon shown, however, that, owing to the marked toxic action of atoxyl, it would not be available in the treatment of syphilis generally, and its use was quickly abandoned. Other preparations of arsenic, however, were suggested. Among these were the so-called "arsenoparapate" of Laveran and Loeffler, the

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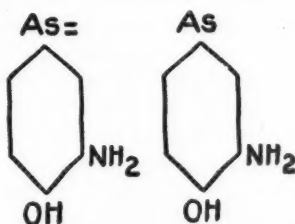
Read before the Medical Section of the Academy of Medicine of Toronto, November 8th, 1910.



dyes "brilliantgrün" of Wendelstadt, and "trypanrot" of Ehrlich. All these substances were given with the idea of producing an "inner disinfection," in cases of protozoan or bacterial infection.

When it had become evident that atoxyl would not be of use in the treatment of syphilis, Ehrlich, following the idea already spoken of, thought of introducing acetic acid into the atoxyl molecule, giving rise to the preparation arsacetine. This substance, however, was found to be very toxic also, and it had to be abandoned. The next arsenical preparation, hectine, prepared by Mouneyrat, and used also by Balzer, was then tried. This is a salt of the acid benzo-sulphone—para-amino-phenil arsenic. It showed a very remarkable activity against luetic lesions, and appeared to be less toxic than atoxyl or arsacetine. No cases of optic nerve trouble were reported from its use. Ehrlich then prepared another compound which has been used by Alt and other German syphilographers until the appearance of "606." It was arsenso-phenylglycine.

This new remedy, "606," is dioxy-diamido-arsenso-benzol and the graphic formula is represented as follows



It is quite unstable in this form, however, and can only be kept in the form of a double salt of hydrochloric acid, bichlorhydrat-dioxy-di-amido-arsenso-benzol. Finally, it cannot be injected in the form of a bichloride, but is converted into a mono- or bi-sodium salt by adding NaOH just before injecting. It is a brownish-yellow powder, resembling flowers of sulphur, only very slightly soluble in water, soluble in glycol and methyl-alcohol, and also dissolving slowly in a concentrated solution of caustic soda.

Various methods of preparing "606" for injection have been suggested. I shall give some of the more important ones. Herxheimer proceeded as follows: The usual dose was fifty centigrams. This was taken and rubbed up carefully in a mortar and then 0.3 c.c. of a twenty per cent. solution of caustic soda added. Continuing to triturate, 10 c.c. of sterile water were added, and after the powder was well

rubbed up into an emulsion, it was injected immediately. Blaschko's method consisted in rubbing up the powder with a twenty per cent. solution of caustic soda in the proportion of 0.36 c.c. of the soda solution to fifty centigrams of "606"; then 4 to 6 c.c. of hot, boiled water were added, and the emulsion so obtained given by intra-muscular injection. The method of Michaelis is more complex and is as follows: For a dose of fifty centigrams of "606,"  $\frac{1}{2}$  c.c. of ethyl alcohol is added and then slowly 20 c.c. of very hot, distilled water. The mixture is rubbed up thoroughly, and when it has gone into solution,  $\frac{N}{1}$  NaoH is added and then two to three drops of 0.5 per cent. solution of phenolphthalein, when a rose-red colour is produced. After this a small amount of a normal solution of acetic acid is added until the rose-red colour just disappears, when a couple of drops of  $\frac{N}{1}$  NaoH are added and a faint rose-red halo is obtained. The liquid is now to be quickly injected.

It was a matter of general complaint at first among the patients that a great deal of pain was felt at the site of the injection. Wechselmann, by preparing the powder in such a way that the reaction was just neutral, claimed that the injection caused much less pain. Finally, Wechselmann and Lange suggested still another improvement, by the use of which they claimed to have an absolutely painless method. They centrifugalized the emulsion obtained by mixing the powder and just enough caustic soda solution and normal acetic acid to have a neutral reaction, and then took the sediment obtained by centrifugalizing and dissolved it in sterile physiological salt solution.

Finally, I shall give the method which I saw used by Dr. Salmon on seventy-five patients at the Hôpital Cochin in Paris. The dose varied from thirty to ninety centigrams, depending upon the severity of the condition. In this clinic the patient's weight was not taken into consideration. The facts noted were: (1) age; (2) stage of disease and severity of infection as evidenced by the clinical picture, and (3) result of the Wassermann reaction.

The powder is taken, the ordinary dose being fifty or sixty centigrams. It is first rubbed up thoroughly with 0.5 c.c. of a concentrated, twenty per cent. NaoH solution, when a fine emulsion is obtained that will readily pass through a fairly small needle. Four drops of glacial acetic acid are added, then 10 c.c. of sterile water. The mixture is again rubbed up thoroughly, and one drop of phenolphthalein, 0.5 per cent. solution, added when a red colour is produced. Small quantities

of glacial acetic acid are introduced until the red colour just disappears, when 10 c.c. of sterile water are added and the reaction again tested with litmus paper. When there is no change in either red or blue litmus, the neutral point is considered to have been reached, and the emulsion is given by deep intra-muscular injection in the gluteal region, 10 c.c. being given on each side. The injection causes a very slight amount of discomfort at the time. But I have heard patients complain of pain the next day, and for several days following.

One or two other points in regard to the dosage and method of injection may be given. It is recognized everywhere that the earlier the syphilis, the larger the dose necessary, and in resistant cases and those in which the disease has rapidly become general, with marked signs of prostration, a large dose is necessary. One injection is usually all that is required, but occasionally a second has to be given. Only two or three of the patients I saw under treatment required more than one dose. "606" can be given by subcutaneous, intra-muscular, or intra-venous injection. I have only seen it given by intra-muscular injection. Ehrlich has advised that it be given intra-venously where one wishes to obtain almost immediate results, and in severe and obstinate cases. As it is excreted by the kidneys, the urine is to be examined before and daily after the injections. Its use is contra-indicated, according to Blaschko, in very old people, with all persons who present visceral lesions that are non-syphilitic, in renal diseases, in persons with cardiac lesions, or diseases of the vessels, such as advanced aneurism, advanced hepatic, splenic or pulmonary disease, and in all who do not show integrity of the eye-grounds. Finally, it is advisable to proceed with caution, and only when a good history is available, in patients suffering from severe brain syphilis, in recent hemiplegia, in acute and subacute meningo-eucephalitis, and perhaps to intervene in these cases only when they are very grave or desperate, and when quite resistant to mercurial treatment. General feebleness, or a condition of cachexia, is not regarded as a contra-indication.

On the other hand, it is distinctly indicated under the following circumstances: (1) In all cases that are refractory to mercury where no contra-indication exists; (2) when a relapse follows immediately after a so-called mercurial "cure"; (3) in chronic relapsing cases; (4) when an idiosyncrasy exists which completely contra-indicates all medication with mercury; (5) in all cases of malignant syphilis or in secondary or tertiary cases where there is great destruction of tissue and mutilation is likely to result; (6) where the syphilitic lesion is so situated as to occasion grave symptoms by its presence.

Now, as to the reactions following the injection of "606." The

pain at the site of injection has already been mentioned. This may be troublesome in certain cases. It appears some hours after the injection and reaches its maximum usually in from twelve to twenty-four hours. Local swelling and inflammatory reaction with marked redness of the skin may supervene. This is particularly likely to be the case where "606" has been given by intra-muscular injection. There is also a slight rise of temperature, and this is especially likely to happen when the injection is intra-venous. Employing Wechsellmann's method, there is frequently no rise of temperature beyond a fraction of a degree. One case in every four or five is likely to show a slight rise if methods of preparation other than Wechsellmann's are used. In some cases a rise of temperature to 102.5° and 103° F. has been observed. Sometimes skin eruptions occur, seen as a congestive halo about the secondary lesions. Constipation usually follows, and occasionally diarrhoea. When large doses have been given, dryness of the throat and other signs of arsenical poisoning have been observed.

In order to follow the progress of the treatments with "606," it has been advised that a Wassermann reaction be attained in all cases, and where possible a search made for spirochætes by means of the ultra-microscope. The results of the Wassermann reaction in cases treated have been somewhat conflicting. Schreiber and Hoppe have observed a disappearance in ninety-two per cent. of the cases treated. Many observers are agreed, however, that it slowly and regularly becomes negative after from three weeks to two months. A longer period of observation will be necessary before a final statement can be made in regard to this. As to the examination for the spirochæta-pallida, it not infrequently happens that they will disappear from the local lesions in from twenty-four to thirty-six hours, but they sometimes are found until the seventh day. Sometimes, before they have disappeared entirely, if they are examined by means of the ultra-microscope, they show much less vitality, their movements are diminished, and later they show granular changes.

I wish next to speak of some of the results obtained in various stages of syphilis, then to consider the matter of toxicity, and, finally, to discuss the question of when "606" should be used in combination with mercury.

First, then, as to the results when "606" is used in primary cases. As a rule, chancres genital or extra-genital show marked changes after twenty-four hours, and sometimes are almost completely cicatrized in four days. In other cases, the chancres have almost disappeared in two or three days, and the cicatrix is formed in six days at the latest. Inguinal glands that have been invaded are sometimes less prompt in

reacting. Yet the results in primary cases, while striking, do not at all compare with the really marvellous change so frequently seen in later stages of the disease. It is possible that in such primary cases the remedy has not free access to the region, and does not come readily in contact with the spirochaetes. Blaschko has recommended, in cases where there is any local persistence owing to ulceration, etc., excision, cauterization, or local calomel treatment,—in other words the local treatment frequently followed.

It is in secondary syphilis that the most remarkable results have been, and are being, obtained. Lesions of the mucous surfaces perhaps show the most striking changes: mucous patches in the mouth, on the tonsils, or soft palate, disappear completely in from twenty-four to thirty-six hours after the injection, and even deep ulcerations and erosions show no trace whatever of their former presence after such a short time. The first thing that impresses one upon seeing a few patients treated with "606" is the rapidity with which it acts. That stands out above everything else. One next is impressed with the fact that there follows a complete disappearance of the local lesions. Local secondary lesions of the genital organs, that owing to an irritating discharge have continued for some time, are usually cleared up completely in ten or, at the most, twelve days, and without using any local treatment other than ordinary cleanliness. Other secondary lesions such as papules, etc., disappear also with great rapidity, sometimes only a slight discoloration remaining after three days.

If, however, the results in such secondary lesions are striking, the results in late and malignant secondaries are still more remarkable. All symptoms in such cases,—headache, loss of weight, fever, loss of appetite, and stupor,—clear up in a manner which must be observed to be appreciated. One such case I saw at the Cochin. The patient was a man of thirty-five years, who had very grave, general syphilis in the late secondary stage. He was dull and stupid; almost comatose; pupils reacted sluggishly; knee-jerks greatly exaggerated; had a profuse secondary rash; was markedly emaciated; had a laryngitis, with difficulty in swallowing, and probably pharyngeal involvement. It was a case of generalized syphilis in which the central nervous system was seriously involved. Despite the fact that a very rigorous course of mercurial treatment had been given, there was no improvement. Sixty centigrams of arseno-benzol were given by intra-muscular injection, and the results entirely justified all that had been said for the use of "606" in desperate cases. Within forty-eight hours there was much less difficulty in swallowing, the rash was waning, and the great, general disturbances, headache, etc., showed much improvement. Within a



week I saw this man sitting up at the long table in the ward, eating his dinner with the other patients.

Another illustration will show still further the value of the new remedy in just those cases that have been least amenable to any treatment up to this time. A young man of about thirty years of age, with syphilis of several months' duration, had been under mercurial treatment, local and general, with no abatement of symptoms, which were as follows: extremely marked cachexia; skin dry and harsh; a large bleeding ulcer over one eye, which quite excluded the possibility of opening that eye; unable to sit up in bed owing to marked prostration; would cry out in agony if anyone used pressure on his arms or legs. One could hardly have seen a more pitiable result from a luetic infection. He was given sixty centigrams of arseno-benzol. After three days he was able to open his eye, submit to pressure without complaint, sit up, move his arms and legs, and the ulcer was drying up and showed a distinct paling.

I could enumerate many similar cases, but these will serve to point out why the German, French and English syphilographers have been most impressed with the results in the so-called intractable cases. In the tertiary stage, where ulcers are so likely to resist treatment, "603" has been found of value, and the rapidity with which gummata have disappeared has been almost incredible. Cases of periostitis have also quickly shown radical improvement, and the relief from pain within twelve hours has been most grateful to the patients. Such general results have almost always been obtained in less than a week. A case cited by Lesser in Berlin is as follows: the patient had two enormous, contiguous gummatous ulcerations in the middle of the dorsal aspect of the tongue; there was a commencing degeneration of a sclerous character; the pain was so great that only liquid nourishment could be taken and that with difficulty. The patient was given sixty centigrams of arseno-benzol, and after eight days the lesions were totally and radically cured.

In parasyphilis of the central nervous system, that is to say, in general paralysis and tabes, and in other cases of brain syphilis, Ehrlich has positively interdicted the use of arseno-benzol. Despite this fact, there have been certain cases in which the treatment has been used, and in one instance at least, a fatal result followed. On August 25th, Fränkel and Grouven reported the case of a young man suffering from a cerebral gumma. Forty centigrams of "606" were given intra-venously. Death followed three and a half hours after the injection. The powder was diluted in only 15 c.c. of physiological salt solution, and the patient presented signs of arsenic poisoning. Those who have given similar



and even larger amounts by intra-venous injection have always diluted the dose in at least 150 c.c. of liquid and have seen no ill effects. This patient, of course, may have had a special idiosyncrasy for arsenic.

Other untoward results have also been reported. Those from Prague appearing in the daily press have received the most attention. There acute renal trouble followed the use of the drug. But the method of preparation has been criticized, and the methyl alcohol used in dissolving the "606" is believed to have been also at fault.

As to the toxicity of the substance, it has been found that 0.1 gram per kilo kills lower animals, and that about seven grams per kilo is toxic for man. I have already mentioned the elevation of temperature and dryness of the throat sometimes observed. Nausea and vomiting have also been caused, although the material vomited showed no trace of arsenic. In very large doses, such as one gram, the heart's action has been affected, and marked irregularity of the pulse observed. With certain other cases a tendency to syncope has been reported. Alt, one of the first German syphilographers to use arseno-benzol, has advised that a dose of fifty centigrams be used, and that this be not surpassed even with robust individuals. He also thinks it wise to give only one injection, and in cases where a second is found necessary, to wait at least fifteen days before giving the second injection. Iversen, of St. Petersburg, who has used intra-venous injections, has reported very encouraging results. He injects slowly 250 c.c. of liquid into the median basilic vein. No pain and usually no other inconvenience have been observed by him, though a slight rise of temperature follows.

From what has been said, it is evident that "606" is a remedy that has certain attendant dangers when it is used in cases where Ehrlich himself has advised against its use. Unfortunately it is toxic for the body-cell as well as for the parasite, so that the high hope of producing a remedy only parasitotropic has not been realized.

Finally, the questions arise, Does this new remedy actually cure syphilis? Does it prevent all possibility of recurrence of symptoms? And is it to supplant the treatment we have so long depended on? Some of these questions cannot be answered as yet, and perhaps not finally for some time to come. In the first place, relapses undoubtedly do occur. Sometimes, after a single injection has caused a disappearance of symptoms, there has been a return a few weeks later. This has been observed by Wechselsmann, who explains it on the ground that the lighting up is due to the presence of spirochetes in various foci of the body, more or less inaccessible, which make their escape later, with a consequent return of symptoms. Since it is true that relapses do occur, complete sterilization or disinfection of the organism is not always

obtained, as Ehrlich had hoped might be the case, though it would seem possible in many cases. The question of its relation to mercurial treatment can be quickly answered. Ehrlich has not advised that mercurial treatment be abandoned. On the contrary, from what has already been said, it will be seen that there is a distinct field of usefulness still left for mercury. It is the expressed hope of those in a position to give an opinion, that the most fruitful results will be obtained by a combination of the old and the new treatment. With a certain proportion of cases responding quickly to mercury or showing an idiosyncrasy to all arsenical preparations, one can readily see that mercury is not to be entirely replaced.

No one can yet answer that most important question—Is the syphilis finally cured in all the cases in which there is such a marvellous disappearance of symptoms? A certain length of time must elapse in order that we may learn how many cases show a return of symptoms. The undoubtedly toxic effect of "606" on the spirochaetes, their granular disintegration following its use, their disappearance from many foci, are all exceedingly hopeful signs, and if the spirochaeta pallida were found only in the blood stream, the problem would likely be solved. Such, however, is not the case; therefore we must wait. In the meantime its use has everywhere been proclaimed with the greatest amount of enthusiasm. The most extravagant claims have been advanced, not by Ehrlich, but by others less well-qualified to speak. The watchwords of that master scientist have from the first been: We must wait and see.

In closing, I should like to quote from an article by an eminent French syphilographer, in reference to the work of Ehrlich: "Whatever be the future fortune of '606,' the discovery of Ehrlich, one can accord one's greatest admiration to this splendid man, whose whole scientific career has been a model and an example of ingenuity, of logic, and of perseverance. The discovery of '606' is not due to a lucky hazard; on the contrary it is the ultimate outcome of a series of researches conducted with great rigour of method and an admirable certainty of deduction. It is better than the spontaneous creation of a purely intuitive genius; it is the crowning effort of a splendid, scientific career and of long years of labour."

## THE EARLY DIAGNOSIS OF PULMONARY TUBERCULOSIS

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**B**Y way of apology for introducing this familiar subject I put forward its tremendous importance. Its importance confronts us, as medical men, in the interests of medical science. Probably, also, it is the most important matter which is affecting the civilized world.

In dealing with the question, I shall attempt to coördinate the facts of pathology, symptoms, and clinical features, which are recognized as of the greatest value in accomplishing our object. The whole question of the pathology of pulmonary tuberculosis depends upon the channel of infection; and, after infection, of the means by which the disease is spread.

We agree that infection may be primarily by the air or by way of the blood; but the vast majority of cases of primary pulmonary tuberculosis are by way of the air through the lymph channels. The older view of Calmette, reviewed by Whitla, is, that the majority of cases are contracted not by inhalation but by the ingestion of bacilli or their products, which penetrate the intestinal mucosa. Whitla and Symmers conclude that the bacillus is carried through the lacteals to the thoracic duct, thence to the jugular vein, and through the heart to the capillaries of the lungs, where they are arrested. Ravenel believes that the commonest mode of entrance in the human subject, except in the case of young children, is by way of the tonsils and the lymphoid tissue of the pharynx.

In all probability tuberculosis is primarily a lymphatic process. If Ravenel is correct, the bacilli pass to the cervical glands; from them to the mediastinal and bronchial glands, and finally into the lung tissue, especially by way of the peribronchial lymphatics of the larger bronchi. From the root of the lung, the paths of infection may be outward towards the axilla to the middle part of the upper lobe, frequently passing to the surface, infecting the pleura, and so causing tuberculous pleurisy, which in turn may lead to a more or less widespread re-infection by way of the sub-pleural and interlobular lymphatics.

Again, infection may pass, not infrequently, to the upper part of the lower lobe. The most common path of infection appears to be outwards and upwards towards that part of the lung close to the verte-

bral column, and midway between the hylus and the apex. This is the usual site of chronic, primary tuberculosis. The explanation of the greater frequency of these foci in the paravertebral, cranial part of the lung is that the tubercle bacilli will grow better in this part, because there is the greatest opportunity for the accumulation of the virus wherever the respiratory movements are slightest. That is to say, if we should introduce at the same time into all the lymphatics of the lungs, the same number of bacilli, beginning with one, and if we should make equal increase, the first infection would arise in the paravertebral cranial parts. It would seem that the chance of infection is inversely proportional to the respiratory movements of the lymph.

If this explanation be a true one, it will also explain other phenomena which are characteristic of chronic, miliary tuberculosis of this region. For example, it will explain why the focus becomes chronic there. It commences from small quantities of virus, and if these foci coalesce, or, by opening into a bronchus, cause a broncho-pneumonic infection, the broncho-pneumonic foci will tend to enlarge before they break down. However, it is sufficient if we assume that the vast majority of cases of chronic, primary tuberculosis commence as a focus situated in the paravertebral, cranial part of the lung.

There is a strong tendency at the present day, in consideration of the frequency of tuberculosis among children, and the evidence of its slow development, to the view that infection takes place during childhood, and that a long period of latency may occur before physical signs or symptoms become apparent. Harbitz gives statistics on this subject, and points out the strong probability of infection in childhood, but admits that the morbid anatomist has not furnished proof of this. In an analysis of 1,000 consecutive autopsies, Adami and McCrae found obsolete or healed tuberculosis in 151 cases, and latent tuberculosis in 93 cases. Out of the 151 healed cases, 100 showed healed pulmonary tubercles, while, of the 93 latent cases, 72 were again pulmonary. Of the 100 healed cases, the site of lesion was the following: upper left, 56; upper right, 48; lower left, 8; lower right, 8; middle right, 5. Of the 72 latent pulmonary cases, single lesions in sites other than the apex were rare. The left lower lobe was affected four times; right lower, three times; right middle but once.

To quote the statistics which Adami and McCrae give regarding ulcerative pulmonary tuberculosis would be unnecessary, as it is our aim to consider this disease at a stage previous to that. The facts which are most striking in the above statistics are: (1) the predisposition to tuberculosis of the upper lobes; and (2) the rarity of single lesions of the lungs in a lower lobe. It would appear that the channels through

which the lower lobe is infected are the air passages and, rarely, the lymphatics.

We are led to conclude that the majority of cases of chronic, miliary tuberculosis affect the paravertebral cranial part of the lung. Chronic nodules in their naked appearance are characterized by a greater translucency than the more acute miliary forms, and a greater fibrous overgrowth surrounding the capsule, which extends into the lung tissue around it, and produces interstitial changes at some distance from the nodule. The centre of the area may be caseous. The microscopic appearance of these nodules is that of fibrous and cellular tissue, with caseation in the centre, and generally with definite giant cells at the periphery. These areas of caseation frequently ulcerate into a bronchus, forming cavities of various sizes. The bronchus becomes infected by tuberculous bronchitis, and its various bronchioles becoming infected, this leads too frequently to acute tuberculous broncho-pneumonia. Haemorrhage may occur from one of these peribronchial nodules, when it takes an active form, and the bleeding or oozing be due to hyperaemia. Or, again, the nodule may rapidly soften and ulcerate, so as to form a communication between a bronchus and a branch of the pulmonary artery, or possibly the tributary of a vein. These are the usual causes of early haemoptysis. But, a beginning focus, under certain circumstances, may develop into any form of pulmonary tuberculosis.

From what we have already considered, it naturally follows that in primary, pulmonary tuberculosis, it is common for the patient to have no apparent symptoms. It is remarkable how little discomfort many people find in various chronic diseases. When one remembers that the commonest initial count in pernicious anaemia is one million red blood cells, it is not strange that in this insidious disease symptoms should not be forthcoming.

Symptoms may be divided into two classes, (1) certain direct symptoms of lung mischief; (2) certain empirical symptoms. I wish at first to direct attention to certain empirical symptoms useful in early diagnosis. Subnormal temperature in the morning is very characteristic of early involvement. At Tranquille during the past summer I was struck with the fact that not infrequently this subnormal temperature on waking was associated with a more rapid pulse than in the evening. This phenomenon disappeared, however, as the patient became better nourished, and as the blood pressure improved. In all probability these symptoms go along with the loss of muscle and the lack of muscular tone which is so characteristic of tuberculous toxæmia. At the same time these individuals wear sweaters, chest protectors, mufflers, and such abominations, in order to make up for their low heat production.



Langour and even anorexia are probably like symptoms, and it would appear that defective hygiene plays a large part in their production.

Anæmia is of special importance in the case of girls and young women. I have seen a number of cases of chlorosis in girls working in the tweed mills in the south of Scotland, and the large amount of pulmonary tuberculosis was very striking. The persistence of the anæmia was oftentimes associated with the appearance of symptoms directly betraying lung mischief. We are accustomed to think of mitral stenosis as being associated with chlorosis, but I believe that chlorosis is more often associated with pulmonary tuberculosis than with mitral disease, and that it is not at all uncommon to find the three present. In British Columbia I have seen about forty cases of tuberculosis. Two of these were young men of twenty, presenting a fair amount of anæmia, with mitral stenosis, and fairly advanced tuberculosis of the lungs. It has been shown that in a series of cases of persisting chlorosis, seventy-five per cent. of those presenting no evidence whatever of tuberculosis gave the tuberculin reaction. These cases were not true chlorosis, as chlorosis does not occur in men. Zickgraf, who makes the above observation, believes that tuberculosis is responsible for a large number of cases of chlorosis. He recommends the tuberculin test to be used in refractory chlorosis.

An attack of *la grippe* is often a manifestation of pulmonary tuberculosis. The patient says that he had *la grippe* when he had, perhaps, a tuberculous auto-intoxication. Gastro-intestinal disturbance, nausea, discomfort, atony, call for chest examination; constipation and gastric disturbances are often due to the tuberculous toxin.

Circulatory enfeeblement is one of the earliest symptoms. It likely keeps pace with the loss of strength which has predisposed to the disease. Thus, the pulse tends to be accelerated and of low pressure, even in the early stages, with the heart presenting a limited area by percussion. On auscultation the first sound at the mitral area will be short and metallic in tone, as of a thin-walled ventricle.

These matters, with other evidence of failing strength, such as incapable chest with delicate bones, may have much to do with predisposition to the disease, because we have seen that the likelihood of deposit of the virus depends upon the flow of lymph in the lungs. Whether one takes to the asthenic or to the toxæmic explanation of these events matters little. On observing their effects, we should look for tuberculosis.

I wish to draw attention to those symptoms which are most commonly noticed by patients themselves. From statistics of cases at Tranquille from January 1st to April 30th, 1910, the symptoms which



induced them to consult their doctor were the following: cough, 67 per cent.; pleuritic cough, 2 per cent.; pain, 8 per cent.; hæmorrhage, 8 per cent.; languor, 6 per cent.; after enteric fever, 2 per cent.; loss of weight, 2 per cent.; asthma, 2 per cent.; pneumonia, 2 per cent. Hæmorrhage is the readiest cause for consulting the physician; next is pain; then cough with sputum. As cough is due either to irritation of the pleura, or to the breaking down of a tubercle causing a bronchitis, the sputum pausing and irritating at the bifurcation of the trachea, or the vocal cords, it follows that cough is usually a somewhat late symptom of the disease. In the majority of cases it requires ulceration for its determination.

Pleuritic pain may be an early symptom due to infection of the pleura by way of the subpleural lymphatics from a chronic nodule, or by a spread directly from the root of the lung. Hæmorrhage as an initial symptom is potentially the earliest of all, as it may come from hyperæmia, or from a very minute ulceration. It would appear, from the statistics at Tranquille, that pleuritic pain and hæmorrhage are more likely to go with early diseases, than symptoms which are not reckoned by the patient as of much importance.

With regard to the clinical features of chronic, miliary tuberculosis, it is also significant that there may be none noticeable. J. J. Galbraith gives statistics of 156 consecutive cases of which twenty-six per cent. manifested hæmoptysis with no physical signs. I do not wish to detract from the great value of inspection, palpation, mensuration of the chest, but I do not wish to deal with methods which may bring out nothing in very early cases. After having inspected the chest from the various points of view, and perhaps having suspicions raised by means of observations taken from palpation and mensuration, one usually passes to percussion. Although it has not the far reaching use of auscultation, percussion, theoretically and practically, is of great value in the diagnosis of very limited lesions. Remembering that the common site of a lesion is at a point midway between the hylus and the apex of the lung, along its vertical border, we instinctively direct our attention to this spot. In auscultation of this region, the bronchus coming near the surface in the right side, and being deeper on the left side, the respiratory murmur is distinctly altered, and may, especially in a patient with thin-walled chest, seem to warrant the diagnosis of infiltration at this spot on the right side. Furthermore, vocal fremitus is normally increased and vocal resonance is normally greater over the right apex behind.

Again, as I have endeavoured to point out, in the pathology of the disease, the pleura is very frequently affected and thickened, and thus again the respiratory murmur is modified. In this way, with an infiltra-

tion of the right apex involving and thickening the pleura, the respiratory murmur, the vocal resonance, and vocal fremitus may not appear in any way, or to any degree, altered, and may not warrant to the mind of the auscultator the diagnosis of a lesion. I am convinced of this from experience of *post mortem* examinations in hospital cases where a lesion was never anticipated, and it has been found that, in all probability, pleural thickening and adhesions over the right apex have covered up and rendered impossible the diagnosis. This condition is analagous to those cases of thickened and adherent pericardium which only appears to muffle and render indistinct the cardiac sounds in cases of chronic Bright's disease and general amyloid disease.

Percussion presents none of these difficulties in the lung, and when we use the tidal phenomenon, as a means of diagnosis, we have a precise method. With the patient sitting on a stool, we place the finger across the trapezius on a plane parallel to the mesial plane of the body. Comparing the note over the one trapezius with that of the other, we are given a general idea of the resonance in the vertical depth of the lung. The value of this procedure, and in fact of all modes of percussion, depends upon the degree in which the pleximeter finger is part of the vibrating surface. The finger must be firmly pressed into the trapezius so as to become part of the vibrating surface. Assuming that this procedure makes us suspicious of one side, we compare the resonance of each apex, and, more important, make a comparative test of the tidal value of each apex. Laying the finger parallel to the clavicle, we try to obtain resonance one inch and one-half to two inches above the clavicle. In deep inspiration this resonance should increase to the extent of half an inch on both sides in front and behind. In the pathology of this chronic disease, miliary in character, it was seen that although the nodule is commonly situated at a point some distance from the apex, it causes increase of fibrous tissue, and, in fact, a form of interstitial pneumonia all round it. Therefore, in an involved apex, the fibrous tissue determines the loss of the tidal movement. Percussion of the bases as well is of value to determine the tidal value of the lung. Furthermore, percussion, with varying degrees of strength of the blow, will often bring out slight degrees of dullness of an apex.

In conclusion, I wish to emphasize, (1) the lymphatic nature of the disease in its primary state; (2) the predisposition of the para-vertebral cranial parts of the lung; (3) the rarity of single lesions in the lower lobes or bases of the lungs; and (4) the importance of percussion. I would also note the valuable use of the von Pirquet test in all questionable cases. It is an undoubted advance over the uncomfortable, subcutaneous injection with its accompanying fever.

## X-RAYS WITH BISMUTH TEST MEAL IN DIAGNOSIS OF GASTRIC CONDITIONS

By F. A. C. SCRIMGER, M.D.

*Montreal*

THE x-ray examination of the stomach with the help of the so-called bismuth meal has been attracting, especially during the past two or three years, a good deal of interest, as is shown by the rapidly increasing literature on the subject; and it is held in some of the larger clinics, particularly the surgical clinics, in which this method is almost always checked by subsequent operation, to be a trustworthy guide in the diagnosis of certain stomach conditions, and—what is often of equal importance—of excluding the stomach as the site of a lesion in other cases.

Some six years ago it was suggested by Rieder that by giving fairly large quantities of a bismuth salt along with a quantity of semi-solid food, the outline of the stomach could be studied by means of the x-rays and photographic plate, the bismuth salts making the stomach, with its contents, more dense to the rays than the surrounding organs. Following the work of Rieder, but emphasizing the importance of the fluoroscopic examination as opposed to the radiographic alone, Holz-knecht, of Vienna, described, in 1906, the outline of the normal stomach shadow as found by him. Much of the work done since has been devoted to determining its normal position, shape, and size, its normal peristalsis and motor power.

Holz-knecht holds, as essential to a normal stomach, that the pylorus should be its deepest point. He admits that he found this shape in only about twenty per cent. of clinically healthy people, the others having an elongated, vertically placed stomach, with a more or less raised pylorus. Most writers find this percentage still smaller and hold that the elongated, more vertical stomachs, reaching to, or nearly to, the umbilicus, with the pylorus somewhat above its deepest point, must be considered among the normal.

The peristaltic movements are seen, under the fluoroscope, beginning as shallow grooves along the greater curvature, deepening to a point a little short of the pylorus, where they are joined by those along the lesser curvature to form a deep ring separating a large cavity towards the cardia from a small pyloric sac, and this, then contracting, forces the food into the duodenum.

The patient is examined in the erect position, with the stomach empty, before the fluoroscopic screen to make sure that the field is clear. He is then asked to drink rapidly three to four ounces of a watery suspension of bismuth carbonate. This is followed as it passes down the oesophagus, usually stopping a moment at the diaphragm, before entering the stomach, where it spreads out, collecting a few moments later at the greater curvature. It is then pressed with the hand towards the pylorus, and a small quantity can generally be seen to pass through into the duodenum. This is of some importance since, later, when the pylorus has closed in response to the stomach activity, it will be less easy to identify the exact site of the opening. He is then asked to eat about nine to twelve ounces of a soft food, containing an ounce and a half of bismuth carbonate, of the consistency of thick porridge. He is again placed before the screen and the shadow studied, particular attention being given to the shape, size, position, and regularity of the outline. The stomach is manipulated, the pylorus identified, and the peristalsis watched. He is asked to draw in and relax the abdomen, any changes in the outline being noted. A plate is taken, and the patient is sent back to the ward, to return in sometimes three, always six, hours for a second fluoroscopic examination and plate. Should the bismuth be still visible in any quantity, he is again seen nine hours after the ingestion and, if necessary, the next morning.

The advantages of this routine are that the time required for the examination is not great; it can be carried on along with the regular work of the department; no complicated apparatus is required and the expense is not excessive, being only that of the plates and the wear on the tubes. It permits the fluoroscopic observations to be compared with those shown in the skiagrams and preserves a record of the cases.

A report of normal, that is, normal so far as the examination by x-rays is concerned, is returned when the stomach is found approximately in the position shown in plates I and II, the outline regular, regular peristalsis seen, and the stomach empty after six hours.

On the form in which the food mass enters the fundus and the rate at which it reaches the deepest point, as showing the presence or absence of normal muscular tone, a very interesting paper has been published by Schlesinger who holds that, with normal muscular tone, the food on entering the cardiac end proceeds slowly in the form of an inverted cone, till it reaches the deepest point only after from five to ten minutes, and that the loss of tone is indicated by a rapid descent of the food, as into an empty sac; and, further, that this is the first indication of a condition which will end in ptosis.

If the bismuth shadow is still present in from six to nine hours

after the ingestion of the meal and no other gross changes are found, the motor power is held to be insufficient. This is admitted to be a somewhat arbitrary division, and really means that the food mass is retained rather longer than what has been found to be the average time, without there being grounds to suspect a mechanical stenosis. Should reverse peristalsis be observed, or should the peristaltic wave be seen to cease constantly at a certain point, this relative retention would be considered of more moment.

The severer grades of a ptosed or dilated stomach, or a ptosed and dilated stomach, will cause little difficulty. The diagnosis of the lesser grades will depend on what is considered to be within the normal limits. At this point it may be well to point out that in estimating the level of the deepest point of the shadow in relation to the marked umbilicus, the position of the tube must be taken into consideration; for a tube placed high up on a level with the cardia will project the shadow well below the umbilicus, when in reality the stomach is in its normal position.

Holzkecht states that he has not seen a case in which the bismuth meal, as given by him, has been retained in any quantity for more than sixteen hours, where there was no mechanical interference with its passage into the bowel; and with such a finding a report of pyloric stenosis is returned.

In the diagnosis of hour-glass stomach the x-ray examination is of undoubted and real value. The picture is most characteristic. The two sacs of a well marked case show clearly, with sometimes a space, sometimes a narrow, ribbon-like band between them. The degree of narrowing, the site of the constriction, and the relative size of the two parts are clearly seen. Sometimes, too, the underlying ulcer is indicated by a deep pocket of darker shadow. Under the fluoroscope the deep, peristaltic contractions can be readily distinguished from the permanent narrowing of the pathological condition.

The alterations of the shadow, as seen in malignant growths of the stomach, depend on two principles: first, that a mass projecting into the stomach cavity, whether at the pyloric end or on the greater or lesser curvature, and being less dense to the rays than the bismuth food mass, will cause clearer spaces or defects in its outline, which, to be significant, must be constant both on the fluoroscopic screen and in the skiagram.

The result of this will be, that, in a pyloric carcinoma, the shadow will appear to end abruptly and more to the left than it should, with sometimes a narrow track leading to the normal position of the pylorus; or, in a growth on the greater or lesser curvature, a defect will be seen in the regular outline of the shadow corresponding to the area of the



tumour. This should theoretically be equally true of a raised ulcer, but the smaller size of the latter will cause it less likely to be shown.

The second principle is, that where deep, crater-like excavations are present the bismuth may lodge and remain there long after the stomach is otherwise empty. On this point the general opinion in the literature seems to be, that, while this picture is sometimes found and has proved in these cases to be a trustworthy guide, it is by no means constant, and cases of ulcer undoubtedly occur where it cannot be demonstrated.

Chronic ulcer may be indicated by adhesions of the stomach to surrounding organs, distortions of the outline and, in deep, penetrating ulcers, a projection of the shadow beyond the regular outline.

Cases of the small, shrunken stomach of diffuse carcinoma have been reported where the bismuth food has been seen to flow directly through the stomach without sign of peristalsis, indicating a contracted, infiltrated wall and a permanently patent pylorus.

Another field in which the method is applicable is in determining whether palpable, doubtful tumours in the abdomen originate in the stomach or in other organs, in demonstrating adhesions, and in fixing the relation of tender points to the stomach shadow.

Very instructive pictures can be seen also in stenosis of the œsophagus, and some work has been done in chronic obstructions of the lower, intestinal tract.

I do not wish to be understood to mean that this method of examination should replace all, or any, of the others, but that it is becoming a recognized and valuable help.

That it is of value in the diagnosis of certain stomach conditions, more particularly the surgical conditions, I believe; but that it will ever prove to be the long-wished-for and long-sought means of diagnosing the beginning of a malignant growth, seems hardly likely.

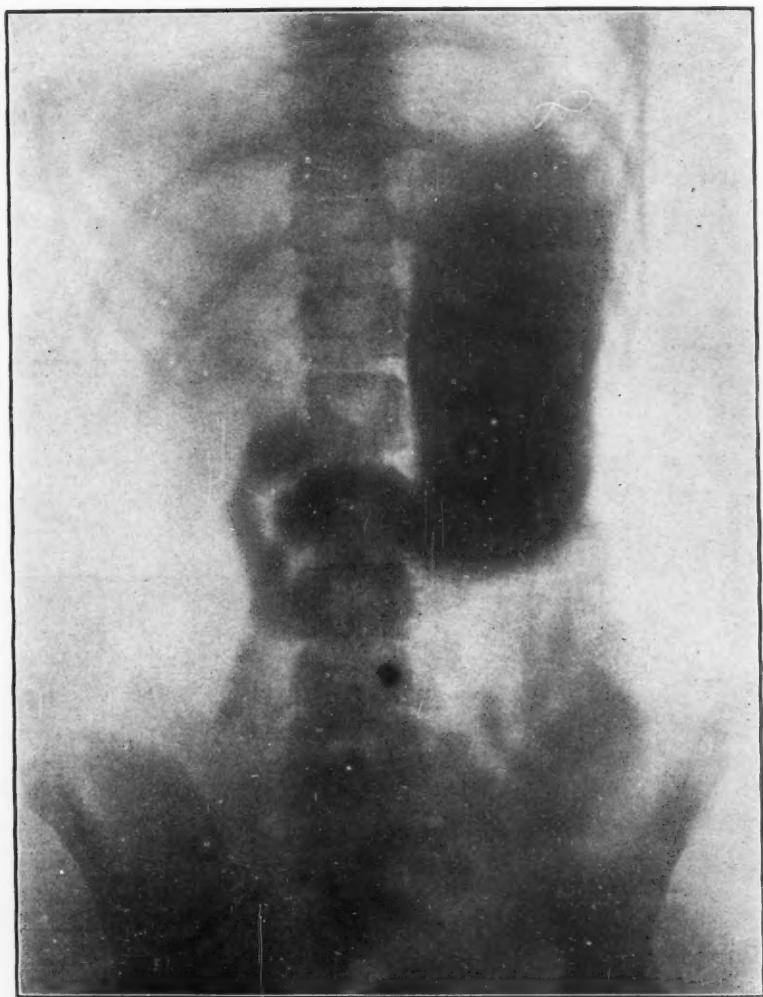
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## Editorial

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### OFFICIAL NOTICE

**T**HE CANADIAN MEDICAL JOURNAL which is issued on January 1st, 1911, is the official journal of the Canadian Medical Association. It is published in virtue of a resolution which was passed at the annual meeting in Toronto, in June, 1910. According to the terms of that resolution the finance committee have appointed an editor. They have entered into a contract with a publisher, and they have acquired the *Montreal Medical Journal*, which is now merged into the new publication.

The Canadian Medical Association is an organization comprising all the provincial associations in Canada, governed by a constitution and by-laws, which were adopted at the annual meeting held in Montreal, September 11th, to 13th, 1907. One of the objects for which the association was established is set forth in the following terms: "The publication of such information as may be thought desirable in the form of a periodical journal, which shall be the Journal of the Association."

Accordingly, THE CANADIAN MEDICAL JOURNAL is issued, as a medium for the expression of all that is best in Canadian medicine. For practitioners of general medicine, or in any special department of it; for workers in the laboratory or the hospital, a vehicle is offered, which goes wide afield, for the conveyance of fresh information, free comment, and sound opinion. There are at present in Canada many excellent journals, some of which are more or less local in

character and circulation; and others which, for a variety of reasons, are more widely known. THE CANADIAN MEDICAL JOURNAL will be scrupulously careful to distinguish itself from these publications, and so avoid any competition which might arise from similarity of methods, so that their interests may not suffer.

It will be its aim to offer adequate means of publication, within the country, of material, some of which, at least, has hitherto been brought to the notice of the profession in English and foreign journals. If Canadian thought upon medicine is to make that impression upon the world, which its importance warrants, it must be concentrated in one channel. What that channel shall be remains for the thinkers themselves to decide.

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### THE MARITIME MEDICAL NEWS

IN the *Maritime Medical News* for December an important announcement is made to its readers, which with all proper humility and thankfulness we reproduce in part.

"There has been, for a long time, a feeling that we in Canada should have a journal, somewhat on the lines of the *British Medical Journal*, a journal which should serve the interests of the profession throughout the Dominion. It was natural that the impulse should come first from the Canadian Medical Association. Whether or not, in course of time, the Association may, as in the case of the British Medical Association, be represented in every part of the country by branches, forming the local medical societies, there can be no doubt that a journal conducted by the Canadian Medical Association, publishing the papers read at its annual meetings, and such other articles as might be contributed, discussing matters of importance to the whole profession, such as medical reciprocity; giving from time to



time a conspectus of medical progress in general, and such items of home and foreign medical news as would be interesting to us all, would be at once an interesting paper and a powerful factor in binding our scattered interests in one Canadian whole. More than a year ago, at the Winnipeg meeting, sanguine spirits hoped for the appearance of such a journal during the present year. There were lions in the way and it is only after an immense amount of hard work and a great deal of mutual concession, that the Finance Committee of the Canadian Medical Association is in a position to issue the circular we have just quoted. Knowing, as we do, the high aims of those who have struggled so hard to found this journal, and the distinguished ability of those directly in charge of it, we have great confidence in urging all our readers to subscribe for it. And this brings us to consider our own relations to the new Journal.

"The *Maritime Medical News* was founded in 1888 by Dr. Arthur Morrow, now residing in Kalispell, Montana, and has, we believe, served a useful purpose, and has been, to some degree, a bond of union to the profession in the Maritime Provinces. It must be evident that the new Journal, if successful in its aims, will serve a much wider purpose and must tend to unify the interests of our profession throughout the Dominion. A careful consideration of all these circumstances has led the shareholders and editors of the *Maritime Medical News* to the conclusion that it is their duty to further, as far as possible, the interests of the new Journal, as they believe their interests are also those of the medical men of Canada, and they have, therefore, resolved to suspend the publication of the *Maritime Medical News* with the current issue. If, at some future time, it should appear to be in the interests of the profession in these Maritime Provinces to have a journal representing local and special needs, we have no doubt men will be found able and willing to resuscitate and revivify this slender body of ours. And now to all our readers a kindly farewell! To

the new Journal our hearty and loyal good wishes, and to all our comrades from Sydney to Victoria (and not forgetting Newfoundland) a Happy New Year!"

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### RECIPROCITY IN TEACHING

THE agreement recently arrived at by the medical faculties of McGill and Toronto, whereby they accept each other's examinations up to and including that at the end of the fourth year of the five years' curriculum, is a step which is bound to count in a marked way in the development of medical education in Canada. The agreement provides that any student who leaves either institution with a clean bill of health, academic and moral, shall be accorded without further examination the same standing as he had in the faculty which he left. To safeguard both universities it is indispensable that he shall have passed all the examinations of the year of standing which he asks the other institution to recognize *pro tanto*. A student who satisfies these requirements could, for instance, spend his first year in Toronto, the second in McGill, the third in Toronto once more, the fourth in McGill, and graduate either there or at Toronto at the end of the fifth year.

It is unlikely that for some years the students of either institution will, in large numbers, avail themselves of the privileges of this treaty of reciprocity between the two faculties. That, however, does not lessen in any considerable degree the immediate significance of the agreement. The chief importance of it lies in the fact that henceforth the two faculties are tending to act as one in the matter of standards of curriculum and examinations. They are still to be rivals, but only in the best sense of that word, for the agreement is a recognition of each other's desire to pursue a high ideal in medical education. In the past the two institutions have

travelled each its own road and singly attempted to do what both only combined could have done. As a consequence of this lack of concert between the two leading medical teaching institutions of the Dominion, not only has there been a great want of uniformity in the requirements exacted for entrance into the profession in the various provinces, but the curricula of the various licensing bodies are behind the times. The medical councils of the different provinces, with every desire to do their duty in this respect, as they understood it, are only qualified in a minor degree, and not at all as bodies of experts, to deal with the question of what a modern curriculum in medicine should comprehend. Their unaided efforts in this direction have hitherto resulted in more or less handicapping the development of medical education; for the medical curricula of the universities must take account of the requirements for the license to practise medicine in the various provinces. Now with the two leading medical faculties united, and with the support of the other medical colleges, a general revision and elevation of the requirements for the license must be undertaken in the very near future, and the result will be an enormous advantage, not only to the profession but also to the cause of medical education.

For the students the advantages of the agreement between the faculties lie in the opportunities it gives them of getting a broad outlook at the beginning of their professional career. With the development on the scientific side which both faculties are striving to achieve and the amplest clinical facilities which McGill offers and Toronto with its new hospital now under construction will give, another order is dawning for medical education in Canada. Either faculty is, therefore, in a position to give a training of the most approved sort; but the value would be greatly enhanced if the students of both took advantage freely of the privileges granted them in the inter-faculty arrangement.

What effect this would have on them may be inferred from the results of reciprocity in this respect amongst the

German universities, twenty-one of which have medical faculties. The German medical student may attend half a dozen universities in his undergraduate career, and he very rarely confines himself to one. The result is that he profits in the fullest degree from the instruction and experience of all the best professors in the Empire, and when he graduates he has a point of view which for breadth cannot be equalled. The Canadian arrangement is, of course, on a smaller scale, but, nevertheless, it enables the student, and without additional cost to himself, to know something of the life at two great institutions of learning. This, in a country like Canada, with its enormous distances and, for this generation at least, the isolation it imposes on a great majority of the practitioners, must prove to be of the greatest benefit in widening his sympathies and stimulating the formation in him of the highest standards of attainment in his profession.

The achievement of this reciprocity arrangement has also led several of its authors to propose an extension of it to some of the leading universities of the Empire. This would enable the student to have all that freedom of transfer amongst the great British universities, that the German student has in the universities of his fatherland. This may, as yet, be a dream, but the successful working of the McGill-Toronto alliance may, sooner or later, result in the development of an Empire-wide university *camaraderie*.

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### QUEEN'S AND THE CARNEGIE REPORT

IN Publication No. 2, December, 1910, issued by the medical faculty of Queen's University, the Carnegie report on medical schools receives trenchant criticism. The faculty considers that the report has been proved to be frequently inaccurate in its statements of conditions, and that, if the inspection of other schools was made in as

superficial and rapid a manner as, it claims, was the case at Queen's, many incorrect statements must have been made and many impressions received not justified by facts.

Although piqued, the faculty believes that it is justified, in view of the hurried inspection, in not taking the report very seriously. The inspection of Queen's by Mr. Flexner and Dr. Colwell occupied part of an afternoon. The registrar of the university gave these gentlemen some general information, and it was only by chance that the dean accompanied them through the General Hospital and the medical buildings. In the evening Mr. Flexner and Dr. Colwell were entertained at the club, and it was after midnight when they left Kingston. It was this untimely departure, the faculty thinks, rather than the earlier dinner, which gave them such a gloomy impression of the future of Queen's medical faculty.

The passages in the report to which objection is taken are those which criticise the clinical opportunities of the school,—“the clinical facilities are limited;” “obstetrical cases are too few;” “the opportunities for out-patient work are slight;” “the clinical years require much more [material] than the town now affords.” These statements are considered as fairly reasonable but as not representing the exact conditions. In their report, Mr. Flexner and Dr. Colwell set down the average number of beds as eighty, these being in the Kingston General Hospital. Upon these beds, the report states, the school relies mainly. The faculty points out that in the General Hospital there are over one hundred and fifty beds, with practically all the patients available for clinical purposes; in the Hôtel Dieu, about the same number, at least one-half of whom may be used for teaching purposes; and at the Rockwood Hospital, over five hundred patients, all of whom are available for clinics. As for obstetrical cases, the faculty notes that the report does not show that there is any school where the obstetrical cases are considered sufficient in number, and claims that Queen's students have as many as those of other schools.

Referring to the paragraph of the report in which it is suggested that the future of Queen's depends upon its ability to develop "the Ann Arbor type of school," the faculty contrasts the conditions of the two schools,—Ann Arbor, 14,000 inhabitants, one hospital, 389 students: Queen's, 20,000 inhabitants, three hospitals, 225 students—and claims that "A more careful inspection of our clinical work, both as to amount and methods, would prove that we do a great deal more for our students than is done at Ann Arbor, and do it equally well. The faculty has recognized for a long time the limitation of our clinical facilities and has, through organization . . . gradually increased the number of cases available till now we consider the number adequate." Dismissing, in two curt sentences, the suggestion that Queen's might maintain a two-year school, the faculty is inclined to place more importance on the fact that Dr. Colwell has included Queen's in Class A, since this gives the school "its proper place and standing from a general view of its equipment and facilities, and does not presume to estimate its future from what could only be a most superficial appreciation of local conditions."

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#### HALIFAX MEDICAL COLLEGE

THE report on medical education in the United States and Canada presented to the Carnegie Foundation for the Advancement of Teaching, by Mr. Abraham Flexner, is a document of very great interest and of permanent value. Rumour has from time to time come to us of the flimsy character of the education given in some medical colleges of the United States, but in the pages of this report we read of conditions almost incredible, and it is made quite plain that there are in that country medical colleges whose teaching is a sham. The fearless and outspoken way in which the report deals with these will, we trust, lead to their extinction.



The report urges many reforms; indeed, it may be said that its proposed reconstruction of the basis of medical education is revolutionary, for it advocates the reduction of the number of centres of medical study in the United States, now over one hundred and fifty, to thirty. But reformers are frequently over zealous, and revolutions are seldom carried out without inflicting injury to some who do not deserve it.

From various quarters protests have been heard against the sweeping statements of the report. We believe that in some cases injustice has been done, and in one case we know it has been done. Nowhere has the report made more stir than in Nova Scotia and in the other Maritime Provinces, where a large number of the medical practitioners are graduates of the Halifax Medical College and of Dalhousie University. The report states that the Halifax Medical College "has no function," and scornfully questions the value of the Dalhousie degree in medicine.

The Halifax Medical College began its career nearly fifty years ago as a medical faculty of Dalhousie University, but there were difficulties in the way of securing a government grant in aid of the medical faculty of the university, while a grant could be secured for the college as an independent institution, and so a separation was effected. Still, a more or less intimate relation has continued; the teaching in biology, chemistry, and physics is given by the university, and the great majority of graduates of the Halifax Medical College take the M.D. degree of Dalhousie University. With the insinuation against its *bona fides* the university is quite able to cope, but its relation to the Carnegie Foundation must determine its reply and its future action.

In view of the fact that so large a proportion of the medical men in practice in Nova Scotia are graduates of this school, and among them are many practitioners of outstanding ability, it is small wonder that a feeling of surprise and resentment should be caused by this report, coming as it does with very weighty authority, and going, as it will, in the pages of

magazines and newspapers, to all parts of the country. All these men are discredited in the eyes of the public; all present students of the college must fear that their time has been thrown away; and that they cannot face the public with the credentials of a school so disparaged and condemned.

For a dignified and trenchant criticism of the report, as it affects the Halifax school, we may refer our readers to a paper read by Dr. D. A. Campbell at the annual meeting of the Nova Scotia Medical Society, published in the *Maritime Medical News* in July, and reprinted in the *Montreal Medical Journal* in its September issue, together with the discussion of Dr. Campbell's paper, and the resolution adopted by the society to the effect that "the report is prejudiced, inaccurate, and misleading," and that the school should be continued.

We proceed to show reason why the report was criticised as "prejudiced, inaccurate, and misleading." The dissecting room is described as "ordinary," and "ill-smelling." The Halifax Medical College makes no claim to be superior; it claims to do good, average work with ordinary apparatus. As to the charge of being ill-smelling, we have not heard that a dissecting room should have the atmosphere of a rose garden. We have never in this country, in Europe, or in the United States, visited an inodorous dissecting room, and we are quite sure that the great anatomists of the past, who laid the foundations of the science, would not have been dissatisfied with conditions in the Halifax Medical College, where the dissecting room is large, well lighted, and well ventilated. The ill odour is to be regretted; it seems to have checked further investigation into the anatomical department. Such investigation would have shown that there is an abundant supply of properly prepared and injected cadavers, that every student receives a full set of bones (a fact carefully noted to the credit of the wealthy Cornell), and that the professor of anatomy, a methodical enthusiast, devotes several hours every day to the work of his department. The disparagement of the anatomical department is perhaps the most bitterly resented part of the report.

The laboratory is pronounced "utterly wretched," and this uncalled for epithet is repeated and reiterated in other parts of the report. Expressions of this sort do not indicate serene judgement, but they make the report more lively reading. The laboratory is by no means wretched. The room is amply large enough and better lighted than the average room devoted to microscopic work. It is true that three classes are conducted in it, but not at the same hour, and there is no interference. The report mentions that the laboratory is connected with the Provincial Board of Health, but fails to note the great advantage of such a relationship. Plain, even meagre, the apparatus may be, but all the essential work of a laboratory is carried on there, from the preparation and examination of morbid tissues and the culture of micro-organisms to the preparation of vaccines.

In discussing laboratory teaching the report classes the Halifax Medical College with such "colleges" as the Mississippi Medical College which does not "own a dollar's worth of apparatus of any description whatsoever"; with the Chattanooga Medical College, where with one hundred and twelve students the dissecting room has only two tables and the "laboratory," one microscope; with Toledo which "has nothing which can be dignified with the name of laboratory," and where "no charts, bones, skeletons" are in evidence; with the Detroit Homœopathic College which has thirty-four students and thirty-five teachers, and laboratory facilities described as "wretched," and with Kansas where the dissecting room "did duty incidentally as a chicken yard."

Then the charge is made of mercenary motives. In over eighty per cent. of the medical schools of North America the income depends on fees alone. In the great majority of these a large portion of the income is paid to the instructors. And until the public is alive to the importance of medical education, and willing to endow medical teaching and research, there is no other practicable method. It is evident that the system is one which may easily degenerate into the worst commercial-

ism, as it has evidently done in the United States, and we are glad to see the courageous way in which the report denounces these bogus colleges.

But there is a great gulf between the Chattanooga Medical College, for example, where the "dean" draws more than a third of the total fees, and the Halifax Medical College where the average "honorarium" to the instructors does not come to \$100. The commissioners who visited the Halifax Medical College did not make themselves acquainted with the personnel of the teaching staff, or they would have seen the absurdity of supposing that men with so many calls on their time could be tempted by the paltry dividends accruing from fees, and these often spontaneously and cheerfully held back for years while the money was being applied to the improvement of the college.

The attitude of the Carnegie commissioners to the Halifax Medical College and other institutions calls for comment, however, on other grounds than these. It is not in this country only that the question of medical education is approaching an acute phase. The raising of the entrance qualifications, the vast expense of modern laboratories, the prolongation of the course, are all proofs of the conviction that our methods are not perfect. And everywhere men enter into "great argument" about this great subject, but evermore come out by the same door as in they went. And the door is that of exchequer.

The report admits the "patent fact that students tend to study medicine in their own states." There are geographical, as well as financial, reasons for the existence of the Halifax Medical College. It is situated eight hundred miles from the nearest first-class centre of teaching in Canada. It is a small school in a comparatively poor country. And the government is not yet ready to endow medical education. Perhaps, if they could absorb the ideas of the Carnegie Foundation, they may yet do so. Until then the question of finance must govern the situation.

It is difficult to speak of this subject without being misunderstood. We are in hearty sympathy with the idea of the commissioners to secure the most efficient teaching of medicine, and perhaps still more with the endeavour to raise the standard of the preliminary education of those who enter on the study of medicine. But some of us are inclined to think too much stress is at present laid on elaborate furnishing and extensive apparatus, too much demanded of the student in what may be called transcendental details, for the demonstration of which expensive apparatus is necessary.

In medicine, as in any other science, the student must take the word of the teacher for some things: the years of a Methuselah are not sufficient to verify all his facts for himself. We believe very good teaching may be given with very little apparatus. Many of the smaller colleges may be inadequate in outfit, yet not, therefore, incompetent.

A few years ago the professor of physics in Dalhousie University carried on his work with a scanty supply of apparatus which would have been sadly inadequate in the laboratory of a Kelvin or a Kohlrausch. But with this slender equipment Gordon MacGregor not only taught and inspired men who now hold important positions in many places, but did original work of a stamp that won the admiration of the leading physicists of Europe and America, and placed him in the chair made illustrious by Tait in Edinburgh.

While we admit to the full the defects due to local conditions, we feel that the verdict of the report is unfair, and we fear it has dealt the college a fatal blow. No student will continue to come, no parent will send his son to an institution so discredited. And without students and the fees they bring the college must fail.

In our opinion there is only one door of hope. Is the door open for a return to the university of Dalhousie? Can the university raise the endowment? Will the medical college regain its ancient nest amid the green and vigorous branches of that sturdy tree, or, bowing to the *delenda est* of



Mr. Carnegie, droop and die? This is a question which every graduate of Dalhousie or the Halifax Medical College, who has a dollar in his pocket, may help to answer.

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### STYLE IN MEDICAL WRITING

**I**T has not escaped notice that all important scientific observations have been recorded with a singular fitness of words. The best scientists have been the best writers upon science. Huxley, Tyndale, and Osler are good writers of English, because the style of each is inseparable from the man. His way of writing is part of himself, since a pen is not a machine which proceeds automatically, once it is set in motion. It must be governed by a hand which, in turn, is directed by a mind.

There are three kinds of writers: those who never think at all; those who think only as they write; and those who have thought before they take the pen in hand. Careful observers belong in the last category, and that is the reason why they write so well. Poor writers employ phrases. Good writers employ words; and they compel each word to give a proper account of itself. They put it to the question. They scrutinize it with the same care as they exercise towards the fact which they propose to describe. If it is weak, or worn, or superfluous, they cast it aside, as a good craftsman rejects imperfect material. Their writing, then, has symmetry and strength, and fitness for the work which it is intended to perform.

On the other hand, loose and slatternly writing is a mark of a slovenly habit of mind. Sheer laziness will produce the same result, since easy reading is hard writing. The trade of the pen must be learned, and he who is not willing to practise it, at first for the fire, has no right to take into his hand that obstinate and refractory tool. It is easy to pour out a flux of words. It is difficult to range them in order, and



make them obedient to the purpose for which they were created. When a man spreads over ten pages what is really exhausted by five, his offence is due to lack of perception, to incapacity or unwillingness to take pains, to indifference to the time and feelings of others,—in short, to sheer selfishness or downright laziness.

But, fortunately, most men, with the exception of editors, are not compelled to read. They can protect themselves by throwing down the book and refusing to open the paper. Practitioners of medicine are too intelligent by nature and too critical by experience to mistake prolixity for learning, and the employment of loose phrases for knowledge. An orator may be diffuse: a writer, in a publication which makes a pretension to being scientific, must not. Presumably, most men desire to have their writings read. Then, they must pay the price.

There is probably more bad writing in medical journals than in any other kind of periodicals. For this there is a variety of reasons. Medical men are without leisure, and there is so much in medicine about which something may be written, that they lose their way. Besides, it is a common delusion that the mere fact of attendance for four or five years upon lectures in a faculty of a university confers upon a man those qualities of aptitude, percision, and harmony, which are commonly called style. On the contrary, the pursuit of a single, dominating interest, as Mr. Asquith told the students of Aberdeen University, limits a man's breadth of outlook and the range of his intellectual curiosity; it dulls his zest and diminishes his eagerness to know and integrate into himself the best that has been thought and written for the enrichment of his mind. In short, it is a bar to the perception of what is good and what is evil in the art of writing.

Much of the technical business of medicine is handiwork. A man may be a most expert operator in surgery, and yet be no more qualified to set forth his methods in plain terms which all may understand than a craftsman can explain by

written symbols how he tinkers a kitchen clock. But a man who is intelligent enough to be a surgeon is also intelligent enough to learn how to write down what he wants to say in simple, accurate terms.

Accordingly, in medical journalism, the great standards of English speech have been lost sight of, and there is a consistent effort to translate plain terms into a kind of jargon. It is, no doubt, a convenience in the laboratory or in the out-patients' department that a jargon should be employed; to refer to the tubercle bacillus as the "t.b.c."; to describe a diseased joint as "a Charcot"; inability to retain the eyeballs in convergence as "Möebius present"; and the absence of a certain reflex as "a Babinski." The employment of these terms in writing is a mark of a slovenly and slow-working mind.

What is worse, many writers appear to think that a good style may be achieved by the use of flamboyant language. The following will serve as an illustration: "Life is a biologic function. Living matter has a definite chemic composition, wasting by oxidation and reintegrating by the assimilation of new matter. The specific feature of living matter is its chemic mutability, and which, *mutatis mutandis* is a history of food changes plus the mechanisms by which its potential energy is converted into vital force." Or, again, "the disequilibrium associated with the cessation of ovulation and menstruation is a menace to mental integrity." Also, there are many phrases which, although quite proper in themselves, have become worn and threadbare by constant use, and their continual employment destroys all sense of freshness. Such expressions as "ætiological factor," "pathological findings," "diagnostic significance," "clinical picture," "symptom-complex," were once good; but they have long since fallen from their high estate.

A careful comparison of the older issues of medical journals with the newer ones, forces one to the melancholy conclusion that, in respect of style, the old were better than

the new. The fault is largely with the younger writers. There are a few amongst them who have that capacity for good writing which is commonly found in the contributors of many years ago, and is still found on the editorial pages of the Journal edited by Dr. John Ferguson, himself a veteran.

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### CAISSON-DISEASE IN CANADA

THE rapid growth of bridge construction and tunnel work in Canada is directing attention to the cause and treatment of caisson-disease. We note in the November number of the *Maritime Medical News*, an article on the "Ætiology of Caisson-Disease," by Dr. P. Conroy, of Charlottetown. Dr. Conroy, for three years, was medical attendant to several hundred caisson workers employed in the construction of the Hillsborough bridge, at Charlottetown, and as the caissons for that structure were sunk to a depth requiring, at times, more than three pressures of atmosphere, he had an opportunity to study carefully this malady.

One of the most interesting points brought out in Dr. Conroy's paper is the fact that the caisson workers have learned by experience the best method of treatment, and will not brook any interference on the part of the doctor. When a workman develops the disease, he is quickly taken charge of by his comrades, and placed in a hot bath. A course of vigorous rubbing is begun, and kept up until the patient has recovered, or until further treatment is deemed unavailing. The convalescent is then kept quiet in a hot room for several hours.

His study of the conditions under which the men work, and the order in which symptoms of the malady develop, have convinced Dr. Conroy that caisson-disease is a toxemia due to excessive catabolism. The facts which he cites are interesting, not only as in support of his conclusion, but as throwing light upon the peculiar conditions under which work in

caissons is carried on. In the midst of the hot, compressed air of the caissons, combustion is very rapid, so rapid that large candles of special make have to be provided. The consequence is, that when workmen first enter the caisson, they experience a wonderful activity, and a stimulus to exertion, due to the excess of oxygen in the atmosphere. This means a more rapid combustion of body tissues, an increased catabolism, the waste being in excess of that which the kidneys under normal conditions are called upon to remove. The skin is then taxed with part of the extra work of elimination, being favoured in its efforts by the high temperature and absence of evaporation. The workmen, indeed, consider that free perspiration is an important and valuable safeguard against an attack.

"When the change takes place to a normal atmospheric pressure," to quote Dr. Conroy, "with distinctly lower temperature, and the evaporation of a large amount of moisture, perspiration ceases, and the kidneys are left alone to do the work of eliminating the waste products so much in excess. Even after the person emerges from the caisson, excessive combustion still continues, kept up by the increased amount of oxygen within the circulation. When the kidney happens to be inadequate through disease or disorder, an abnormal amount of toxic material remains within the blood, giving rise to the different nervous phenomena commonly met with in uræmic conditions."

The fact that an attack may come on while the worker is within the caisson strengthens Dr. Conroy in his belief that the disease is chiefly a toxæmia. He has noted that workers have learned by experience that there is an increased liability to the disease when the bodily functions are disordered and that they discourage one another from going down when suffering from systemic disease. A significant aspect of the article is that, if the disease be a toxæmia, the workers have themselves hit upon the best treatment,—inducing profuse perspiration, and stimulation by large draughts of coffee.

### Editorial Notes

A curious case came up in the Ontario courts before Mr. Justice Riddell in December. Counsel on behalf of a physician who had been charged before a magistrate with having performed an illegal operation, and acquitted, applied to the court asking that the Ontario College of Physicians and Surgeons be prohibited from proceeding with any investigation into the charge. Judge Riddell decided that inasmuch as the legislation upon which the case rested gave the college the right to enquire into any case where a doctor was accused either of a criminal offence or of "infamous and disgraceful conduct," the college had the power to investigate a criminal offence, when it was also professionally infamous misconduct. The law merely prevented a further investigation in another criminal court, and was clearly meant to give the Medical Council power to proceed with an enquiry, whether or not a court conviction had been made.

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On Saturday, November 26th, 1910, at the York Club, on the occasion of his retirement from the professorship of gynæcology, in the University of Toronto, a large number of the many friends of Dr. Temple tendered him a banquet, and presented him with an address and silver tea service, as a mark of their high appreciation of his valuable services to medical education and to the profession. Dr. Temple has been a teacher of medicine for forty years; first, in Trinity medical school, and, later, after it was merged in the medical faculty of the University of Toronto. During all that time he has done distinguished and disinterested service to his profession. It is gratifying to know that Dr. Temple is still to continue active in the discharge of his professional work.

## Res Judicatae

### THE NEW TREATMENT OF SYPHILIS

SO many new facts concerning syphilis have crowded upon us of late years that a readjustment of our ideas concerning this disease has been rendered necessary. Animal inoculation, the spirochæta pallida, the Wassermann reaction, and now "606," have so altered our former concepts that there is some danger of losing those teachings of our elders, without a true recognition of which our later knowledge is, and will be, far from complete.

It is, therefore, in season that in respect to Ehrlich's new compound, attention should be called to the fact that syphilis in the past has been, on the whole, well treated by mercury and potassium iodide, and that, where intelligently used, it has been the exception, rather than the rule, for these drugs to fail. Nevertheless, it must be admitted that such cases do occur, and in sufficient numbers to cause us to welcome gladly any such discovery, or better, any such invention, as Ehrlich has found. Whether, at this date, we can go so far as Neisser, and declare that every case of primary or secondary syphilis should be advised to undergo treatment with this compound, our experience is as yet too limited to permit of a conclusion. Nevertheless, the series of cases referred to below, would seem to strongly support this view.

From the literature so far published—almost exclusively German—little has been learned as yet beyond the fact of the compound's amazing power to alter syphilitic lesions for the better. In one, five, ten years from now, we shall be able to speak with authority. At the moment, it would be folly to draw conclusions in a disease of the chronic and relapsing character of syphilis. For the present, therefore, all must observe and work. More recent papers, it is true, tend to bring out the less brilliant aspects of treatment with "606." Undoubtedly, cases of recurrence occur: the *summum bonum* of a *therapia sterilans magna* is not yet assured in man, whatever experimentation on animals may show. This does not mean that this end is unattainable. The most important point in "606" therapy is that again and again it has been shown to succeed where mercury has failed, and such a fact alone deserves all the enthusiasm which has been evoked by this compound.

By the generosity of Professor Ehrlich, through Dr. Simon Flexner, opportunity has been given to treat some fifteen or twenty cases in the genito-urinary clinic of the Montreal General Hospital. It is not



improbable that a more detailed report of these cases may be contributed to this journal in the near future. Suffice it, therefore, to give a more or less general idea of results, in so far as an opportunity has been had to observe them.

Two men had been under treatment for a period of six months, by means of intramuscular injections of metallic mercury. Though the disease got no worse, it also improved but little, largely owing to the lack of proper hygiene. At the time of admission to the hospital, one suffered from multiple mucous patches of the mouth and lips, and had numerous large squamous plaques on both legs; while the other had similar patches in the mouth, and, in addition, an anal condyloma. Twenty-four hours after an injection of "606," a distinct improvement in the patches was noticeable, and within five days these had practically disappeared. The anal condyloma had likewise vanished, and the squamous plaques were dry and healthy, though one was still adherent, and the pigment was somewhat darker.

Three cases of primary syphilis with secondary eruptions, were also treated. Within three days, the induration had, to a large degree, left the chancres, and the surface had a healthy, healing appearance, and subsequently healed rapidly. The effect was less marked on the secondary eruption, which visibly improved in the first forty-eight hours, and then more gradually, so that some evidence could be found two or even three weeks later.

Two cases of primary sore, without secondaries, were also treated. In one of these the sore completely disappeared within one week. Two cases of serpiginous syphilide, one of the foot, the other of the forehead, were treated by "606." In the space of two weeks, the one disappeared, while the other had almost gone. Changes were also noted in a case of rupia, which altered greatly in forty-eight hours, and in a broken down gumma of the palate, though the time elapsed does not permit of further remarks. The bone pains in a case of sclerosing osteitis disappeared in twenty-four hours.

There is, perhaps, no need to add further cases, particularly if it has been made evident that "606" has a very definite action in certain lesions of syphilis. All of the above cases were diagnosed by the finding of the spirochæta pallida, or by a positive Wassermann reaction. Among the most remarkable phenomena noted was the rapid disappearance of the spirochæte. In cases of primary and secondary lesions, where, previous to injection, these could be found in enormous numbers, twenty-four hours afterwards none could be found, in spite of careful and prolonged search.

Of unpleasant phenomena, the pain was the most constant.

Temperature, an erythematous eruption, gastric disturbance, and a prolonged soreness and stiffness at the site of injection, were noted in one or more instances. Of changes in the Wassermann reaction nothing can be said, owing to the short period of observation.

In these present days, when the press and the popular monthly magazine have brought "606" before the public, both in Germany and America, it is incumbent on us to be doubly cautious in drawing conclusions as to its efficacy. It is, therefore, with the greatest conservatism that these cases are referred to, though the facts are very striking. According to Professor Ehrlich, not only does the compound act by killing the spirochæte, but also by producing some antibody. Evidence of this is seen in the change in the Wassermann reaction, but especially in the fact that nursing mothers, who have received "606," can cure their syphilitic infants by continuing to nurse them. And yet the quantity of arsenic in the milk is scarcely recognizable. There must, therefore, be some other body, antagonistic to the spirochæta pallida, which effects this improvement.

There is still much that we need to know. What is to be the future of these cases, even if, after one or two doses, no further signs develop, and the Wassermann reaction becomes negative? What relation must the size of the dose bear to the virulence and stage of the disease? Are mercury and potassium iodide to be used along with "606" in order to make assurance doubly sure, or can "606" be solely relied upon? "606" is a brilliant discovery, as brilliant as antidiaphtheritic serum, but it has to cure a disease which is the most chronic and persistent of all diseases, and it behoves us to be doubly cautious in our conclusions. There is a certain definite *begristerung* of the public mind, not to speak of the medical world, and it is not improbable that some published results are affected thereby. Let us keep an open mind, but in all thankfulness and in all admiration of a great achievement, in all recognition of what "606" has done, and to what it promises to do, let us have patience to note its actual results, favourable and unfavourable. Syphilis is not yet harmless, is not even on its last legs. Would that it were, but it has surely received a serious blow.

## Book Reviews

**APPLIED ANATOMY.** By GWILYM G. DAVIS, M.D., Associate Professor of Applied Anatomy, University of Pennsylvania. Octavo. 600 pages. 575 illustrations in colours and black. Cloth, \$6.00. J. B. Lippincott & Co., Philadelphia. Canadian agent, Chas. Roberts, Lindsay Building, Montreal.

This book is a most ambitious one, and of large size,—too large for an ordinary text-book on applied anatomy, but excellent as a work of reference for practitioners. According to the preface, "its aim is to show the relations of structure to function, whether it is normal function, or function disturbed or impaired by injury or disease." It is also partly a work on operative surgery, for many operations are described and illustrated. A brief account is first given of the skeleton and muscles of the region; and then follows the surface anatomy, and afterwards the affections and accidents the parts are liable to, the nerves and blood vessels being described where important. The work is most beautifully illustrated by original drawings of the highest finish and of the clearest characters, from preparations made by the author and his assistants. There are illustrations derived from other sources which are duly acknowledged. We can heartily recommend this most comprehensive and accurate work, and consider it a valuable addition to the many works on applied anatomy.

**PATHOLOGICAL ANATOMY PLATES, REPRODUCED FROM FRESH PREPARATIONS, WITH EXPLANATORY ANATOMICAL AND CLINICAL TEXT.** By ALFRED KAST, M.D., Professor of Clinical Medicine, Hamburg; assisted by EUG. FRAENKEL, M.D., Professor of Anatomy, General Hospital, Hamburg-Eppendorf and THEODOR RUMPEL, M.D., Senior Physician, General Hospital, Hamburg-Eppendorf. English Translation by FRANCIS C. PURSER, M.D., Dub., F.R.C.P.I., Assistant Physician, Richmond, Whitworth, and Hardwick Hospital, Dublin; with preface by J. LORRAIN SMITH, M.D., Professor of Pathology, University of Manchester. Complete in twenty-six parts at \$1.25 each. London, Bailliere, Tindall & Cox, 8, Henrietta Street, Covent Garden, 1910.

These handsome life-size plates, accurately reproducing in colours the appearance of different organs in the more important morbid states, will be found indispensable in schools not provided with an adequate pathological museum, and even where such a museum exists, the difficulty in preserving the natural colour of specimens will make the separate

plates of this atlas of great value for teaching purposes, not only in pathology, but in medicine and surgery. We are indeed struck by the singular beauty and accuracy of the illustrations, and the moderate price at which they are furnished, and can only speak of this new atlas in terms of the highest praise.

**THE PRACTICE OF MEDICINE, A GUIDE TO THE NATURE, DISCRIMINATION AND MANAGEMENT OF DISEASE.** By A. O. J. KELLY, A.M., M.D., Assistant Professor of Medicine in the University of Pennsylvania, 1 vol., pp. 945, illustrated: Lea & Febiger, Philadelphia and New York, 1910.

This volume, which, as stated in the preface, is intended for the student and the junior practitioner of medicine, is compiled on lines very similar to those of the usual text-book on medicine, of which so many have been issued from American, and especially Philadelphia, publishing houses in the last ten or fifteen years. It claims "to devote most space and attention to the practical aspects of medicine; to the elucidation of those principles exemplified in disease at the bedside, in hospital wards and clinics, and in the consulting room; and to the clinically important, and more common, disorders rather than to the rarer diseases, however interesting." It is but fair to state that this determination to keep to essentials has been duly adhered to, though a carping spirit might take exception on this score to the short sections on nocardiosis, Nasha fever, ponos, Japanese river fever, kubisagari, gangosa, and goundou, which savour more of the "curious" than of the "practical" aspect of medicine from the standpoint of the student and junior practitioner of medicine. Short sections on pathological physiology, preceeding the descriptions of diseases of a system or conventional group of diseases, are a special feature of the volume. In this the author seems to have been inspired by Krehl's "Clinical Pathology," as may be seen by comparing pages 213 and 214 of Hewlett's translation of this work with the author's page 510. The sections on treatment are full, clear, and judicious, though marred by the inclusion of a few formulæ "as an aid to the junior practitioner." The title-page bears the word "illustrated"—a qualification hardly justified by the interpolation of some temperature charts,—taken, with one exception, from other writers,—three woodcuts of tape-worms, and a short dozen of the usual plates and figures illustrative of the gross and minute anatomy of the nervous system and cerebral localization. One might summarize by saying that the work is an excellent compilation, but asking at the same time—is it necessary, and does it fill a long felt want?

A SYSTEM OF SYPHILIS. By D'ARCY POWER AND J. KEOUGH MURPHY.  
Vol. V., Oxford Medical Publications: Henry Frowde, Hodder  
& Stoughton, London; Canadian agents, D. T. McAinsh &  
Co., Toronto.

To those familiar with the preceding volumes of this system, it is enough to state that Volume V. is not behind its predecessors either in the manner of its production or in its contents. To these and to others who, as yet, are unfamiliar with these volumes, it is necessary to add that, in Volume V., the cutaneous lesions of syphilis are considered by Phineas Abraham, and Haldin Davis, of London; syphilis of the eye, by Devereux Marshall; aural syphilis, by C. Ernest West; and syphilis of the upper air passages, by St. Clair Thomson. More than any of its precursors, it is a volume of specialties, and, as such, demands a welcome from the specialist rather than from the general practitioner. He, however, who is interested in this disease, as an entity,—and who is not?—owes a very definite debt to the various authors who have collected the many scattered facts of this disease in the various regions of the skin, eye, ear, nose, and throat, and have placed them in such a form that ready reference is possible. To a certain extent, therefore, each section deserves criticism from the standpoint of the specialist rather than from that of the syphilologist, if we may use the term. In general, the reading matter is concise, systematic, and clear; references abound, and the bibliography is, on the whole, comprehensive.

In the first part, which deals with the skin, one notes that that much confused subject, the classification of syphilides, receives due consideration. A simpler, and if less descriptive, arrangement is advocated,—truly the one way out of a maze of terms which, though sonorous and learned, teach little.

The importance of the aid furnished by the *spirochæta pallida* and the Wassermann reaction appears throughout the volume, yet the authors are eminently conservative, (perhaps too much so), in their demands on these tests.

The plates and illustrations, which form an integral part of any treatise on the skin, are here excellent. No other work can show more exact and beautiful representations of skin lesions, and few can equal it. Though the other parts of the book lend themselves less readily to illustration, yet the abundant plates representing numberless affections of the eye and its fundus, ear, throat, and nose, deserve more than a modicum of praise. Such plates fortunately, or unfortunately, will outlast much of the accompanying text, in spite of the fact that it represents the last word on these subjects. In the past few years such last words

have had frequently to be altered after the lapse of a few months. So in this very volume, after a casual word on Ehrlich's "606," the editors have had to append a page with additional details, none too exact by the way.

The system, of which Volume V. forms a part, is undoubtedly the most complete and excellent which has yet appeared in English or, perhaps, in any other language. Its greatest fault is its expense, which to a certain degree must limit its usefulness. It is eminently English in thought and expression, in spite of contributions from such foreign writers as Elie Metchnikoff and Castellani, and perhaps for that reason is the more to be welcomed at the present time.

**ANATOMY, DESCRIPTIVE AND APPLIED.** By HENRY GRAY, F.R.S., late Lecturer on Anatomy at St. George's Hospital, London. New (18th) edition, thoroughly revised, by EDWARD ANTHONY SPITZKA, M.D., Professor of Anatomy in the Jefferson Medical College of Philadelphia. Imperial octavo, 1496 pages, with 1,208 large and elaborate engravings. Price, with illustrations in colours, cloth, \$6.00, net; leather, \$7.00, net. Lea & Febiger, Publishers, Philadelphia and New York, 1910.

This present, eighteenth, edition of this well-known work, coming so soon after the last, is evidence of its great popularity. This edition is practically the same as the one published two years ago. Many new illustrations, however, have been added, and some of the old ones left out. The revision has been carefully done, some parts have been rewritten and obscure passages made clear. It is useless to praise a work so well known, and so much prized and used, by practitioners and students. The only criticism we should make is that as a text-book of anatomy it is most complete, but of such enormous size that students are staggered by the amount of information which it gives, and feel that it is almost impossible for one ordinary mind to absorb it all. The sections on applied anatomy might well be omitted and published as a separate work, for a beginner can hardly appreciate to the full so much applied anatomy. The illustrations are magnificent and the text excellent. We can heartily recommend this great text-book.



## Retrospect of Medicine

### SOME RECENT PAPERS ON CARDIO-VASCULAR CONDITIONS

JAS. H. HEARD. The significance of transient cerebral crises and seizures occurring in arterio-sclerotics. *Edinburgh Medical Journal*, November, 1910.

ALLAN AND WILSON. Arterial spasm in the brain associated with transient and permanent paralysis. *Glasgow Medical Journal*, July, 1910.

LONGCOPE AND MCCLINTOCK. The effect of permanent constriction of the splanchnic arteries and the association of cardiac hypertrophy and arterio-sclerosis.

FLEISHER AND LOEB. Further investigations in experimental myocarditis.

LOUIS HAMMAN. The heart muscle in typhoid fever. *Archives of Internal Medicine*, October 15th, 1910.

There are few clinical observers who have not recognized at least one or two cases illustrating the conditions referred to in the titles of the papers appearing in the *Edinburgh* and *Glasgow* medical journals above quoted. Apoplectic or epileptiform seizures, aphasia, hemiplegia, monoplegia, paræsthesia, hemianopsia, recurring and leaving no permanent recognizable effects, have been witnessed over and over again. These two papers give brief reports of ten cases illustrating all of these conditions mentioned. There is an autopsy reported by Wilson on one of Allan's cases.

Heard's cases are found in old people from sixty-two to eighty-six years, while Allan's patients, six in number, are much younger, twenty-six to sixty, four being under forty-one years of age. On account of the suddenness of the attacks and the rapid and complete recovery, one cannot attribute them to either hæmorrhage, or thrombosis, or emboli, and thus some other pathological basis must be looked for. Heard remarks that none of his patients showed evidence of being afflicted with either progressive paralysis, advanced renal degeneration, or occasional heart block; each patient was a sufferer from intestinal indigestion and the subject of marked arterio-sclerosis.

Allan, in explaining these crises, adopts the hypothesis of "angio-spasm in definite vascular territories of the brain," as advanced by

Russell and supported by others. Such well-known writers as Brunton, Adami, Krehl, Kaufmann, and Osler, are quoted. As no connexion between the cerebral vessels and the vasomotor centres in the medulla has been demonstrated, it is suggested that the cerebral vessels possess a self-regulating apparatus, and that the conditions of the brain-matter and those of the blood determine the flow. With sclerotic cerebral vessels, sclerosis of other organs is commonly found. Under such circumstances there is a diminished ability to destroy toxins, and an increased tendency to produce them.

Several causative factors are enumerated in Heard's paper: the action of a toxin upon the brain cells in localized areas; a spasmodic, local contraction of cerebral arteries sufficient to interfere with function, but not to affect the integrity of a part; so considerable a decrease in compensatory hypertension that certain areas of the brain are temporarily insufficiently supplied with blood through sclerotic vessels; and localized areas of oedema in the brain substance. Both writers emphasize the view that toxins are active as a cause, not necessarily through a centre, concerning the existence of which there is considerable doubt, but by direct action upon the vessel wall. Dixon and Dale have shown that two substances obtained from putrid meat, which may be produced in the body by the stagnation of the intestinal contents, have an action analogous to, but more prolonged than, adrenalin. Tanzi puts forth the view of a localized poisoning of brain cells to explain the seizures of progressive paralysis. He is quoted as saying, concerning these attacks, "that they are capable of being regarded as manifestations of irritability or exhaustion associated with a morbid action which is temporarily localized in cerebral areas." It is argued that arterio-sclerotic patients cannot withstand fatigue, and it is not surprising that a brain with damaged vessels should be particularly subject to localized alterations of function resulting from a state of intoxication poorly resisted. The angiospasm view is not without its toxic factor.

Since the arteries of the brain have not been found in spasm (Osler), this view receives its main support from clinical analogy. With cases of migraine, Raynaud's disease, anæsthesia, and aphasia, alterations in the senses of hearing and smell may be associated. After discussing this phase of the etiology, Dr. Heard concludes that one point at least may be regarded as definitely determined; namely, that there is a marked analogy between the method of production of an attack of migraine and the transient manifestations of arterio sclerosis and of certain allied conditions as well, and that both are produced by a process of auto-intoxication. The teaching that a diseased hypertonic artery responds to stimuli even more vigorously than does a normal artery, explains the

localization of the lesion, and at the same time closes the argument from the standpoint of angiospasm.

That decrease in compensatory hypertension accounts for a few cases of these cerebral crises seems highly probable from observations made by Miller. In a patient under treatment, transitory left-sided numbness succeeded baths which lowered the blood pressure 25 mm. When still lower, at 190, thirty below the usual reading, the patient developed repeated transitory hemiparesis. Like disturbances followed the use of vasodilators. The high pressure seemed necessary to supply the brain through the narrowed vessel. Cerebral edema is regarded as a rare condition explaining these attacks, if indeed it does occur at all.

That vessel spasm predisposes to the formation of thrombi or of apoplexy has been pointed out by Russell, and one of Allan's cases is reported as bearing this out. Allan and Wilson urged this view of the origin of multiple thrombi in the absence of any source of emboli, and in vessels the walls of which showed no disease. A careful study of their case-report, however, is not convincing on these points.

The problem of cardiac hypertrophy has been again approached from the experimental side, and Longcope and McClintock have observed in animals the changes permanently due to constricting the splanchnic arteries. About thirteen years ago Hasenfeld, and later Hirsch, showed that arterio sclerosis of the thoracic and abdominal aorta and especially a narrowing of the lumina of the splanchnic arteries were potent factors in the causation of cardiac hypertrophy. It is mainly with an experimental proof of this problem that this paper is concerned. These observers desired to reproduce as closely as possible, by experimental methods, the narrowing by arterio sclerosis of the lumen of the celiac axis and the superior mesenteric arteries, and at the same time to study the effect of this operation on the blood pressure and heart. In some previous experiments it was found that constriction of either the celiac axis or the superior mesenteric artery caused an immediate rise of the blood pressure from 8 to 22 mm. with an average of 14 mm. of mercury, while constriction of the two arteries at the same time doubled the pressure, which persisted in some experiments for at least an hour.

In these experiments dogs were used. Under ether the abdomen was opened in the median line. The superior mesenteric arteries and the celiac axis were dissected free from the dense plexus of nerves surrounding them, and Halsted's aluminum bands were slipped round the vessels and tightened until pulsation was almost obliterated in the distal portion of the vessels. The wound was then closed. Sixteen dogs were operated upon, and nine died within two weeks, pneumonia and infarction of the intestine chiefly accounting for the fatalities.

Following out careful records of the observations made, it is evident that there was no definite change in the blood pressure. At periods varying from four to six months the dogs were killed and the hearts weighed. These all were well within the normal limits when compared with the hearts of twenty-four normal dogs as control.

These observers conclude from these experiments that extreme narrowing of the mouths of the superior mesenteric arteries and the celiac axis in dogs is soon compensated for by a collateral circulation. Neither cardiac hypertrophy nor hypertension follows the narrowing of the mouths of these vessels in dogs. The conclusion based upon autopsy reports is in accord with that reached in experiment, that no definite association can be found in man between sclerosis of the abdominal aorta and the great splanchnic vessels and cardiac hypertrophy. We await the results of a study now in progress on the condition of the splanchnic arterioles and its relation to blood pressure.

Experimental myocarditis has occupied the attention of Drs. Fleisher and Loeb for some time. Over a year ago they published a preliminary report of their work, with the conclusion that the typical effect of an intra-venous injection of adrenalin was a cardiac and not an aortic lesion. These workers experimented mainly upon rabbits, and used 0.2 c.c. of a 1-1000 adrenalin solution, after injecting the animal with small doses of spartein, 0.012 gm. per kilogram, or caffein, 0.025 gm. per kilogram. They tested the effects of the second injection of adrenalin on the myocardium, and the effect upon the kidneys of a combination of adrenalin and spartein, and then made several observations on the influence of the myocarditic lesion on the secretion of the urine, on oedema of the lungs, on ascites and on pericarditis.

The conclusions can best be stated in the words of the writers:—

(1) "The injection of one single dose of spartein or caffein with adrenalin causes the appearance of gross myocarditic lesions in sixty per cent. of the rabbits injected, and the appearance of the microscopic lesions in the hearts of almost all the rabbits."

(2) "The lesions appear a few days after the injection. The earliest change (separation of the muscle-fibres due to oedema) may be noted a few hours after the injection. The gross lesion may become apparent a few days after the injection."

(3) "The lesion consists, in the earlier stages, in a combination of the following changes: (a) increase of connective tissue which appears very early and is quite diffuse; (b) hypertrophy of muscle fibres with increase of the double nuclei of the muscle cells and indistinctness of the cross-striation; (c) marked degenerative processes affecting the muscle fibres."

"It seems probable that the interstitial and parenchymatous changes develop independently of each other, at least at first. At later stages parenchymatous degeneration induces connective tissue proliferation."

(4) "In from eight to twenty weeks there is a gradual disappearance of tissue changes. After twenty weeks the changes are represented only by small fibrous areas which contain remnants of atrophic muscle fibres. These areas of fibrous tissue replace degenerated muscle fibres. The connective tissue increase is secondary."

(10) "We believe that excessive mechanical strain is the direct cause of myocarditic lesions. The typical seat of the lesion, at the base of the left ventricle, where the greatest strain is exerted, favours this theory. Furthermore, analogous conditions have been shown to occur in striated muscle in conditions of over exertion. The fact that the injection of spartein and adrenalin into dogs whose hearts are relatively stronger than those of rabbits, does not cause the appearance of myocarditic lesions, adds further support to this theory. These lesions are, in all probability, not due to a lack of nutrition of the muscle fibres as a result of the contraction of the coronary vessels, inasmuch as it has been shown that adrenalin does not cause a contraction of the coronary vessels."

Dr. Hamman's paper is of interest from an historical standpoint as well as for its teaching on the anatomical and pathogenic features of the cardiac lesions in typhoid. He examined forty-three cases and gives ample reports upon all of them. His conclusions are in accord with the teaching of the present day upon the cardiac complications of typhoid fever, that while the heart is affected in a general way by the action of the poison of typhoid fever, serious organic changes are the exception and not the rule, even in those cases where death appears to be due to toxæmia. As he points out, the myocardial lesions found are of the greatest importance for the future health of the individual, and while typhoid fever is not so important in this respect as rheumatism, syphilis or diphtheria, it is, on account of its prevalence, a factor in the production of chronic arterial and myocardial diseases, to be seriously reckoned with. Dr. Hamman found no evidence of widespread change in the smaller branches of the coronary arteries.

#### ANTI-TYPHOID VACCINATION

There has been appearing during the last twelve months an increasing volume of literature on the subject of protective inoculation or vaccination against typhoid fever. This has been largely due in America to the introduction of the procedure into the United States

Army. From this source alone a considerable body of data is already available; no less than twelve thousand men having been inoculated during the period from March 1st, 1909, to October 1st, 1910. The absence of any untoward results in this large number of cases has demonstrated, at any rate, the harmlessness of the procedure. As to its efficacy, army medical officers have been sending in enthusiastic reports. They do not hesitate to claim for it, in individual cases, a degree of immunity amounting to practical protection against typhoid for three years.

The technique of the preparation of the vaccine differs in some essential particulars from that used in the armies of England and Germany. Questions, therefore, as to the standardization of method of preparation will soon present themselves. The question, too, as to its free distribution by government health laboratories is also pertinent.

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### Personal

Drs. R. Monahan, Montreal; J. A. Rousseau, Montreal; and D. A. Kearns, Ottawa, have been appointed surgeons to the Canadian Navy.

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Dr. J. B. Chambers, for four years assistant superintendent of the Brandon Hospital for the Insane, has been appointed superintendent of the hospital at Selkirk. Dr. Chambers is succeeded at Brandon by Dr. Hicks, of Griswold.

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Dr. H. S. Ford, of Vancouver, died while on a hunting trip last November. Dr. Ford strayed from the party, and his body was found several days later in a valley, where he had died apparently from exposure and exhaustion.

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Surgeon-Lieut.-Col. Kenneth Cameron, who has been in command of No. 5 D. Field Ambulance of Montreal, since the foundation of the corps in 1904, has been promoted to the post of Administrative Medical Officer for Montreal district No. 5.



## News

At a meeting of the medical faculty of Queen's University, held on November 19th, a resolution was passed, dealing with the water supply of Kingston. The resolution reads: "That this faculty views with alarm the present condition of the city's water supply, and regrets the loss of time involved in the hitherto ineffective efforts at repairing the intake pipe. It respectfully urges that a new pipe be laid forthwith, and that treatment of the water, by the hypochloride method, be immediately instituted."

Dr. J. L. Rhea, of Boston, has been appointed to the pathological department of the medical faculty, McGill University. Dr. Rhea succeeds Dr. Wolbach, who has gone to Harvard.

As a result of the semi-annual examinations of the British Columbia Medical Council, the following persons have been admitted to practice as physicians and surgeons in British Columbia: Drs. C. S. Bastin, J. H. M. Bell, A. D. Bechtel, M. B. Campbell, H. H. F. Coulthard, D. W. Davis, D. Donald, W. F. Ewing, E. P. Fewster, J. M. Fowler, S. Holman, A. W. Hunter, O. S. Large, H. C. L. Lindsay, W. F. McDonald, W. F. McKay, R. McKenzie, H. McMillan, M. H. McNaughton, J. J. McNaughton, H. B. Marchant, W. D. Murray, J. Nay, K. J. O'Neill, L. A. Patten, S. B. Peele, A. G. Price, A. G. Ridewood, J. K. Shaw, B. Shaw, R. Shimlein, A. C. Sinclair, N. Telfords, S. T. Wilson.

The following practitioners have been elected as territorial members of the Medical Council of Ontario: Drs. G. R. Cruickshanks, Windsor, div. 1; A. B. Welford, Woodstock, div. 2; J. McArthur, London, div. 3; T. W. Vardon, Galt, div. 5; H. S. Griffin, Hamilton, div. 7; W. H. Merritt, St. Catharines, div. 8; R. J. Gibson, Sault Ste. Marie, div. 9; Alex. D. Stewart, Fort William, div. 10; J. S. Hart, Toronto, div. 12; H. Bascom, Uxbridge, div. 13; T. W. G. Young, Peterboro, div. 14; W. Spankie, Wolfe Island, div. 16; J. Lane, Mallorytown, div. 17; M. O. Klotz, Ottawa, div. 18.

The report of the chairman of the Board of Health for Prince Albert shows that the Board has been active during the past year in introducing measures of hygiene. One of the results has been a marked lessening of contagious diseases. For the year 1910, the total number

of cases of typhoid was eighty-three, a decrease of 194 cases as compared with the estimate for 1909. A filtration plant is now being installed in Prince Albert.

The reports of the medical health officers for Ingersoll, Georgetown, and Grimsby, for the year ending November 15th, 1910, have one criticism in common,—the lack of a proper sewerage system in each of these places. The reports show that these towns have been comparatively free from infectious and contagious diseases during the year.

According to the report of the Kingston Board of Health, there were two outbreaks of typhoid fever in that city during the past year. From October 1st to December 12th, forty-five cases developed. Contaminated water was thought to be the cause.

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### Obituary

DR. H. D. AYRE, of Regina, who had his leg broken last year in a runaway, died on December 1st, 1910, from the effects of the accident. The late Dr. Ayre was for twenty years a member of the North-West Mounted Police, and after retiring from that force was connected with the Dominion Veterinary Department.

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DR. JOHN GUNN, who for thirty-five years practised his profession in Ailsa Craig, Ont., died on November 28th, 1910. Dr. Gunn was the father of Dr. John Gunn, of Port Dover, and a brother of Dr. William Gunn, of Clinton.

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The death occurred on November 27th, 1910, of Dr. Arvin Stoddard Leavitt, of Bancroft, Ont. Dr. Leavitt was born in 1833, in the county of Leeds, and studied medicine at Queen's University. He first practiced his profession at L'Amable, in Hastings County, where he was well and favourably known in both social and political life. Seven years ago, he removed to Bancroft. Dr. Leavitt is survived by a son and one brother.

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DR. G. F. EMERY, of Ottawa, died November 23rd, 1910. Dr. Emery was a graduate of Queen's University, '89. He was born in Leeds township in 1866, and married Gertrude Birch, of Kingston. He leaves a widow and three sons. Dr. Emery practised general medicine in Gananoque until 1904, when he went to Ottawa, and gave his attention to diseases of the eye, ear, nose, and throat.

## Canadian Literature

### ORIGINAL COMMUNICATIONS

#### *The Canada Lancet*, December, 1910:

- The Medical Man as a Witness . . . . . W. R. Riddle.  
Chairman's Remarks, Section of the Academy  
of Medicine, Toronto . . . . . R. D. Rudolf.  
Papilloma of the Appendix . . . . . E. A. Hall.

#### *The Canadian Journal of Medicine and Surgery*, December, 1910:

- The Medical Man as a Witness . . . . . W. R. Riddle.  
The Use of Carbon Dioxide Snow in Superficial Lesions, H. J. Todd

#### *The Canadian Practitioner and Review*, December, 1910:

- The Borderland of Medicine and Surgery in  
Diseases of the Kidneys . . . . . H. B. Anderson.  
The Early Diagnosis of Phthisis . . . . . John A. MacGregor.  
Medical "Process Building" . . . . . John Hunter.

#### *Dominion Medical Monthly*:

- Vaccine Therapy; Its Administration, Value and Limitations  
The Relation Between Organic and Functional Nervous Diseases

#### *Le Bulletin Médical de Québec*, November, 1910:

- Notes de pratique chirurgicale . . . . . Arthur Simard.  
De l'examen des urines dans la pratique journalière, A. Vallée.

#### *Le Journal de Médecine et de Chirurgie*, November 26th, 1910:

- De la nécessité de l'ablation des végétations adénoïdes,  
et des amygdales hypertrophiées . . . . . J. N. Roy.

#### *Le Montréal-Médical*, November, 1910:

- Comment relever le niveau de la profession médicale . . . .  
Rottot (Août, 1872).  
L'Enseignement de l'hygiène à l'école . . . . . J. Decaire.  
De la transfusion du sang . . . . . François de Martigny.

## Medical Societies

### MONTREAL MEDICO-CHIRURGICAL SOCIETY

THE fifth regular meeting of the Montreal Medico-Chirurgical Society was held December 2nd, 1910.

#### EXHIBITS

Dr. Maude E. Abbott exhibited a stereoscopic microscope, showing calculi, intestinal parasites, bony tissues. Drs. Abbott and J. Kaufmann showed a complete series of cardio-vascular thrombi, as follows:—

##### Cardiac Thrombi.

1. In septicæmia. Death from general peritonitis.
2. In typhoid. Death on the 28th day.
3. Mural thrombi. In dilatation of chronic myocarditis.
4. Mural thrombi in coronary artery disease. Sudden death.
5. Mural thrombus in cardiac aneurism.
6. Hollow polypoid thrombus in the left ventricle.
7. Thrombus projecting through the mitral orifice. Death from cerebral embolism.
- 8 and 9. Old and recent clot filling the greatly dilated left auricle. Mitral stenosis. Sudden death.
10. Ball thrombus blocking a stenosed mitral valve.
11. Embolism of abdominal aorta from the same case as specimen No. 10.
12. Two ball thrombi in the left auricle. (Extremely rare condition.)
13. Cardiac embolism from thrombosis of the common iliac veins. Sudden death.

##### Vascular Thrombi.

1. Flat thrombus in the aorta, plugging the orifice of the right coronary artery.
2. Old laminated thrombus extending from the thoracic aorta, to the iliac arteries. Ultimate gangrene of the legs.
3. Thrombosis of the left and embolism of the right pulmonary artery. Sudden death.
4. Thrombosis of the femoral and brachial veins. Same case as specimen No. 3.

#### LIVING CASES

1. Hæmatoma of the humerus. Dr. K. Cameron and Dr. A. MacKenzie Forbes. The patient was a boy of ten who, while playing, sustained a fracture of the right humerus. He was brought to the Montreal General Hospital under the care of Dr. K. Cameron, who opened the joint and found the cavity filled with yellowish fluid, some blood clot, and broken bone. This was curetted away, and the joint was put in splints, with the result that the patient has perfect use of his arm. Skiagram showed hæmatoma in the bone.

II. Three years previously this patient had a rusty nail enter the knee. Pus formed; an arthrotomy was performed and the joint was washed out; the wound healed by granulation. The knee became

flexed, but nothing radical was done, as the child was then but seven years of age. The case was handed to Dr. MacKenzie Forbes who, realizing that it was impossible to consider an excision of the knee because of the danger of destroying the epiphysis, decided to make an artificial fracture in the femur. This was done in two stages, and the joint was placed in plaster of Paris. The two skiagrams, taken eight weeks after, showed the femur almost in a line with the tibia and fibula; buttresses of bone were forming from the lower end of the femur to the upper extremity. The boy can now walk comparatively well.

III. Osteitis Deformans (Paget's disease). Dr. C. F. Martin and Dr. L. S. Foster presented this case, which was a typical one, and exhibited skiagrams showing the bowing of the long bones and the enlargement. Dr. Foster is at present studying the metabolism of this condition. This case was discussed by Dr. A. E. Garrow.

#### PATHOLOGICAL SPECIMENS

The following series was exhibited by Dr. L. J. Rhea, of the Montreal General Hospital:

1. Ectopic Gestation. The patient, who had passed one period, was taken with sudden pain in the hypogastrium, on November 13th. Three days later, severe pain was felt in the left inguinal region, with general abdominal tenderness. On the 19th there was slight bloody discharge. The day following, the patient passed a uterine cast. Operation revealed left ruptured tubal pregnancy.

2. Heart with multiple mural thrombi. Patient was in hospital five days, complaining of cough, weakness and shortness of breath. The heart was enlarged, and irregular in action. Autopsy revealed multiple mural thrombi in the left ventricle. On palpitation they were semi-fluctuating. Section of one in the apex of the ventricle showed soft reddish contents.

3. Urethral strictures. Strictures dilated some years ago. Internal urethrotomy four weeks ago. Perineal abscess opened one week ago. Abdominal pain five days later and retention. Perineal cystotomy. Death. Autopsy—Strictures of the urethra, acute cystitis, diverticulum of the bladder.

4. Carcinoma of the prostate. History of frequency of micturition. Examination showed a large, hard prostate, with small nodules. Inguinal glands palpable, bladder distended, stricture of urethra, frequency of retention. Autopsy—carcinoma of prostate. Metastases in bladder and right ureter; hydronephrosis. Note the thickened trigone, obstruction of ureteral openings, nodule in the right ureter, and hydronephrosis.

5. Metastatic carcinoma of vertebræ. Specimen shows two vertebræ in which the bone (red) has been replaced by tumour (white). From same case as specimen No. 4.

These pathological specimens were discussed by Dr. J. M. Elder, Dr. A. Lapthorn Smith, Dr. H. L. Pavey.

#### LANTERN DEMONSTRATION: "Trachoma Bodies"

Dr. Hanford McKee showed a series of slides of the supposed cause of trachoma from cases of trachoma and from normal eyes, which

would seem to throw doubt upon the theory that these bodies are the cause of trachoma.

#### CASE REPORTS

1. Missed abortion, Dr. C. K. P. Henry.
2. Some unusual cases of albuminuric retinitis; and,
3. Cases of tubercle of the choroid, by Dr. G. H. Mathewson.

#### PAPER

The paper of the evening was read by Dr. J. C. Meakins on "The various tuberculin tests, and their application in diagnosis."

Synopsis of Paper. The rationale of the tuberculin reaction in general was discussed with a description of the method of administration, results, contra-indications and dangers of the various tuberculin reactions, viz:—

1. The subcutaneous.
2. The conjunctival.
3. Scarification, or Von Pirquet.
4. Inunction or Moro.
5. Inter-cutaneous.
6. The animal or Yammanuchi.

Those who took part in the discussion which followed were: Dr. J. M. Elder, Dr. A. D. Blackader, Dr. A. E. Garrow, Dr. G. H. Mathewson, Dr. R. H. Hardisty, Dr. W. E. Enright, Dr. J. R. Goodall, and Dr. F. M. Fry. Dr. Meakins replied.

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THE sixth regular meeting of the Montreal Medico-Chirurgical Society was held on December 16th, 1910.

Dr. W. F. Hamilton exhibited a living case of endarteritis obliterans, upon which amputation of the leg had been performed. The patient came under the surgeon's care with a moist gangrene of the leg. He had been working under water at Gaspé laying the foundation for a bridge, and it was thought this was the cause of the gangrene. There was no pulsation in the popliteal vessel at the time of amputation; there was a thrombus in the popliteal artery. Drs. Garrow and Russel discussed the case and Dr. Hamilton replied.

Dr. R. P. Campbell exhibited Goldsmith's Urethroscope, an instrument which, he said, was an excellent one for examination purposes, giving a very satisfactory view, especially of the caput. Dr. M. Lauterman had used this instrument freely; but, though it was an excellent examining instrument, it was useless for treatment. He preferred a modification of Vallantin's, which, while giving a



clear view of the seat of the trouble, could also be used for its treatment. Dr. Lauterman also expressed the opinion that the limitations of these instruments should be properly appreciated

Dr. J. Kaufmann exhibited the following specimens for Dr. O. C. Gruner:—

1. Specimen of pancreas and adnexa from a case of chronic pancreatitis and cholecystitis. Note dilation of the pancreatic duct, common bile duct, and of both hepatic ducts. There was sudden narrowing of the bile passage at the papilla, which is very prominent and hard. The liver substance is dark green in colour from obstruction of the flow of bile. The gall bladder contained a clot of blood, of large size, forming a cast of the cavity. The walls are intensely congested, and show the changes due to cholecystitis.

2. Vegetative aortitis. The specimen consists of the thoracic aorta and shows several prominent vegetations just beyond the arch. When fresh, these vegetations were very conspicuous and bright red in colour. Their outline corresponds with that usual in an atheromatous ulcer. The aorta was dilated. The specimen comes from a man aged fifty-four, who died of typhoid fever owing to perforation of two ulcers. The heart was dilated and hypertrophied. There was no endocarditis.

3. Large solitary cyst of kidney. The specimen consists of the left kidney, the lower end of which is occupied by a large cyst,  $8\frac{1}{2}$  c.m. in diameter. Small cysts are seen near the large one. The upper end is normal. The other kidney showed only one small cyst and was marked by granular. The specimen is derived from a man aged 64, who showed general arterial disease, dilatation and hypertrophy of the left ventricle, and died of cerebral thrombosis.

4. Extensive typhoid ulceration of the large intestine. The specimen shows very many large ulcers with undermined edges. The smaller ulcers are evidently follicular. One of the ulcers at the lower end had perforated. The case was interesting as presenting only symptoms of appendicitis.

5. Heart showing ulcerative endocarditis, with perforation of the aortic valve. There are polypoid vegetations along the free edge of the valve, and on the posterior cusp, they have a cox-comb-like appearance; the flap of this valve shows a large perforation about 1 c.m. in diameter. There was also a hæmorrhagic pericarditis and infections in the spleen and lung.

Referring to specimen No. 1, Dr. James Bell said that the case illustrated one of the difficulties which surgeons had to contend with in determining between a fibroid and a chronic, malignant condition in that region. Some months previously an exactly similar case had come under his care, where the condition was thought to be fibroid, but the condition turned out to be a malignant one from which the patient subsequently died. Dr. J. L. Rea asked if the typhoid bacillus had been recovered from the case of vegetative aortitis. Dr. Kaufmann replied that, although the bacillus had not been recovered from the blood, the signs in the small and large bowel were very definite.

Dr. George E. Armstrong mentioned two cases he had seen abroad where intra-venous anæsthesia had been used, and used successfully. One was an adult with very advanced malignant disease of the tongue and glands of the neck. The other was an abdominal case. Inhalation anæsthesia was first introduced, then the intra-venous to keep

the patient under its influence. This case was also successful. The ether was sent into the cephalic vein at about three feet elevation. Two jars were used, one containing saline solution with five per cent. ether, the other plain saline solution. Tubes from these jars led into a Y, that from the ether jar was allowed to run in, and, in about ten minutes, the patient was thoroughly under its influence. By clamping the tube giving the ether, one could regulate the depth of the anaesthesia, and if much blood was being lost, one could turn on the saline solution; the tubes did not run simultaneously. The patients came out of the anaesthetic promptly. Cases had been reported where thrombosis had occurred.

A report on twelve cases of rectal anaesthesia was read by Dr. F. W. Nagle of the Royal Victoria Hospital. The method, in his hands, had been very successful. Drs. Bell, Garrow and Von Eberts discussed the report, and Dr. Nagle replied.

The open treatment of fractures with lantern demonstration, by Dr. J. M. Elder, was the paper of the evening. Cases suitable for such operation were considered from the standpoint of advantages to the patient, to the public, and the hospital. The choice of material for fixation of the broken bones—Lane's plates, MacEwen's screw nails—end results of such operations, favourable and unfavourable, were all discussed. The paper was followed by the exhibition of a living case, lantern slides illustrative of cases operated upon by Dr. Elder, with a synopsis of each case, and plates, screws, and instruments used for the fixation of broken bones. The paper was discussed by Drs. Garrow, Armstrong, Bell, Hutchison and England. Dr. Elder replied.

Dr. J. Alexander Hutchison read the report of a case of papilloma of the bladder, and Dr. J. L. Rhea exhibited the pathological specimen. Drs. England, R. P. Campbell and Lauterman discussed the case. Dr. Hutchison replied.

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#### TORONTO ACADEMY OF MEDICINE

A meeting of the Academy of Medicine was held December 6th., 1910, in the biological department of the University of Toronto, Dr. A. A. Macdonald, president, being in the chair. The meeting was devoted to a series of papers on the subject of immunity. Professor J. J. Mackenzie dwelt on the general aspects of the topic, and first considered briefly the development of immunity from the fundamentals originated by Pasteur and Koch. The rise of the cellular and humoral ideas, and their development and expansion were also considered,

recent theories were reviewed, and the progress of the past five years was touched upon. The work on anaphylaxis was concisely presented, stress being laid on the possible relationship between anaphylaxis and immunity, as suggested in the more recent work of Friedberger, Sles-wijk, Loeffler, and others. Landsteiner's suggestion as to the rôle of lipoids in various immunity reactions was, according to the speaker, a most suggestive, recent contribution to the subject, and one likely to lead to fruitful results, as experimental work develops.

Professor Amyot reviewed the rise of the practical use of the theoretical considerations in this field, as exemplified in the preparation of vaccines, bacterial vaccines, antisera and antitoxins. The advances in modern serum therapy and the practical advantages of being able to confer passive immunity were dwelt upon.

Dr. Caulfeild's paper, the third on the programme, was on the subject of the application of immunity to clinical medicine. The speaker, at the outset, announced that he purposed limiting himself to the presentation of work done in tuberculosis by himself and his co-workers in the laboratories of the National Tuberculosis Sanatorium, at Gravenhurst. This paper embodied work of a most important character, and can best be explained by reference to previous work in the same field. As is known, Calmette and Massol (*Comp. Rendu de la Soc. de Biolog.*, February, 1909) showed that there exists, in the sera of certain well-marked cases of tuberculosis, substances masking the presence of the sensitizers in these sera, which, in consequence, give a negative reaction of fixation (Bordet and Gengou). This latter observation has been made by many workers on tuberculosis. Calmette and Massol believe that they have demonstrated the presence in these sera of a substance heretofore undescribed and designated by them "inhibiteur." This substance is held accountable for the failure to obtain positive reactions of fixation in known, positive cases. Dr. Caulfeild, by other experiments, and quite independently of Calmette, has been able to show the presence of this substance, called by him "inhibitin," in certain sera. Further, by his technique he has been able to reveal the presence of "inhibitin" in certain definite clinical types and in "clinical normals." By this means, also, it has been possible to indicate, in certain types, the probable outcome of the given case. In other words, in Dr. Caulfeild's hands, the new measure has been found to have great diagnostic and prognostic value. A large number of sera have been examined. The work has been controlled by clinical observations and by the recognized laboratory methods available for the study of tuberculosis.

Dr. G. W. Ross gave a summary of his five years' experience with

vaccine therapy, its indications and limitations. As a pupil and co-worker of Wright, he has been able to acquire an unusual experience. A plea was made for closer coöperation in the clinical medicine, in order that the full worth of bacterial vaccines may be realized.

The discussion of the evening's papers was opened by Dr. J. G. Fitzgerald, who dealt with the observations of Bail on aggressins, and of Cole and Smernow on the same subject. The work of Ledingham, Dudgeon and Shattock, and Nikolsky, in regard to the determination of the opsonic content was also reviewed. The local character of anaphylactic shock, based on the work of Gay, Southard and Fitzgerald, Auer and Lewis, Anderson and Schultze, Mainwaring, and Pearce, was also briefly considered. Observations on the differences between natural and specific sensitizers, shown by recent experiments of the speaker, brought this part of the discussion to a close.

Dr. McPhredran, in discussion, touched on the extreme importance of such work as that of Dr. Caulfeild, and offered him his hearty congratulations. Professor Mackenzie, Dr. N. A. Powell, Dr. Albert Macdonald, and others expressed themselves in a similar manner in regard to the value of this work. Dr. Caulfeild closed the discussion.

#### VANCOUVER MEDICAL ASSOCIATION

THE regular meeting of the Vancouver Medical Association was held November 14th, 1910. Present: Dr. Monro in the chair, and thirty-two members. Dr. Glen Campbell reported for the credential committee; and, as a result of his report, Drs. W. Barrett, J. A. Sutherland, A. L. Johnstone, and S. Paulin were elected for ordinary membership; and Drs. Margeson, Shinbein, Bastin, Peele, Champion, MacMillan, Wilson, and Gray for privileged membership. Dr. Monro, the incoming president, then gave his inaugural address. The various phases of the address were discussed as follows: Dr. R. E. McKechnie, on expert testimony, and regarding the liquor act and coöperation of the druggists to amend abuses; Dr. Underhill, announcing action of council concerning building a morgue, and death certificates,—the inadequacy of the present form; Dr. Gordon, on question of prophylaxis of social diseases; Dr. Proctor, on commission for health department, on interviewing attorney-general to secure legislation about accident cases, on expert evidence, and assessors to advise the judge as to medical evidence. Dr. Burnett spoke on question of state registration of nurses; Dr. Jeffis, on health department, morgue, pathologists and post mortem work,

on medical testimony, and expert witnesses; Dr. Pearson, on securing land to build a permanent house for the society.

It was moved by Dr. Burnett, seconded by Dr. Brydone-Jack, "that the executive be empowered to appoint committees without naming the chairman of each, to look after the following questions and report from time to time to the Association." Carried. Committees were appointed to deal with the following affairs. Commission for health matters; medical expert testimony and assessors; registration of births and deaths; coroner's court and morgue; social prophylaxis; care of drunkards and abuse of hospitals; registration of trained nurses; building for the library.

#### OTTAWA MEDICAL SOCIETY

THE officers elected for the year 1910-1911, are the following: president, Dr. F. McKelvey Bell; first vice-president, Dr. H. C. Church; second vice-president, Dr. C. W. F. Gorrell; secretary, Dr. Omar Wilson; assistant secretary, Dr. Neil McLeod; treasurer, Dr. J. H. Alford; council, Drs. W. C. Cousens, J. D. Courtney, G. S. McCarthy, Lorne Gardner, Neil McLeod.

The Society meets every second and fourth Friday, at 8.30 p.m., throughout the winter months, at St. Luke's Hospital.

The following is the programme for 1911:—

January 13th—"A case of incomplete retention with distention of bladder in prostatic hypertrophy."

Dr. L. C. Prevost. Critic—Dr. H. C. Church.

January 27th—"On some Aspects of Immunity."

Dr. Campbell Laidlaw. Critics—Drs. Small and Higgins.

February 10th—"Carcinoma of Uterus."

Dr. W. Travis Gibb, New York.

February 24th—"Literature in Medicine."

Dr. G. P. Howlett. Critic—Dr. Omar Wilson.

March 10th—"Carcinoma of Stomach with Partial Gastrectomy."

Dr. A. T. Shillington. Critic—Dr. C. W. F. Gorrell.

March 24th—"Differential diagnosis and treatment of acute and chronic nephritis."

Dr. J. C. Caskey. Critic—Dr. Lorne Gardner.

Clinical cases will be reported at every meeting.



## NOVA SCOTIA MEDICAL SOCIETY

THE fifty-eighth annual meeting of the Nova Scotia Medical Society, which is held on the first Monday in July of each year, will, in 1911, be held in Halifax jointly with that of the Maritime Medical Association. The officers are: president, Dr. James Ross, Halifax; first vice-president, Dr. Evan Kennedy, New Glasgow; second vice-president, Dr. Joseph Morton, Shelburne; secretary-treasurer, Dr. J. R. Corston, Halifax.

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THE annual meeting of the London Medical Association was held on December 13th, 1910. The following officers were elected: president, Dr. D. H. Arnott; vice-president, Dr. C. H. Reason; secretary-treasurer, Dr. Edward Spence.

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THE annual election of officers of the Thunder Bay Medical Association was held on November 17th, at Port Arthur. The officers elected were the following: honorary president, Dr. T. S. T. Smellie; president, Dr. A. J. G. Macdougall; vice-president, Dr. M. B. Dean; secretary-treasurer, Dr. C. C. McCullough; executive committee, Drs. J. D. Chisholm and C. Powell. Interesting addresses on medicine were given by Drs. Dean and Pratt, and on surgery by Drs. Crozier and Manion. The attendance was the largest in the history of the society. Those present were: Drs. Smellie, Beck, Martin, McCullough, Wodehouse, Stewart, Cook, Boyd, Chisholm, Brown, Williamson, Crozier, Bucke, Pratt, Macdougall, Hunt, Eakins, Chipman, Grimshaw, McGillivray, Oliver, Dean, Manion, McCartney.

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At the annual meeting of the members of St. Thomas and Elgin County Medical Society, held at St. Thomas on November 18th, the following officers were elected: president, Dr. S. N. Dorland, of Rodney; vice-presidents, Dr. Riddle, Richmond; Dr. Smith, Fingal; and Dr. Cormack; secretary, Dr. Leith; treasurer, Dr. Duncombe; committee, Drs. Crane, Cameron and McEwen. Dr. Coll Sinclair, of Aylmer, read an interesting paper on "The Relation of the General Practitioner to Surgery." There were present: Drs. Spurgeon Campbell, Winnipeg; Coll Sinclair, Aylmer; Riddle, Bayham; Mothersill, Port Stanley; J. D. Curtis, F. Guest, Campbell, Leitch, W. F. Luton, C. W. Marlatt, Geo. A. Marlatt, R. M. Lipsey, Alex. Turner, Duncombe, T. L. Gray, Hill, W. F. Cornett, F. O. Lawrence, McKillop, D. L. Ewin and Cormack.